THE MEDICAL WORLD OF THE EIGHTEENTH CENTURY

The Medical World of the Eighteenth Century

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TO MARJORIE

Foreword

It is a time-honored custom of historians, be they medical or general, to divide the continuum of history into hundred-year periods, and thus to create the impression that a century is a firm historical unit, rather than a by-product of the decimal system. There is little doubt that such divisions are often arbitrary. However, if any century deserves to be treated as a cohesive unit—particularly from the medico-historical point of view—it is the eighteenth. In its course were laid the foundations for what we have come to call modern or scientific medicine.

Although most medical historians have felt strongly about the contributions of this century, their evaluations of its characteristics differ greatly. Baas considered it "the most important century in the history of culture and particularly the sciences," a century of enlightenment that demonstrated its humanitarian idealism in medicine and the allied sciences as well as in politics and philosophy. Pagel shares this enthusiasm and adds that this period was one of emancipation from religious and political domination, when science could flourish, unhampered by any external pressure. Fielding H. Garrison, the eminent American medical historian, summarized the era in terms which are somewhat less rapturous than those of his German colleagues. To him the eighteenth century produced much "tedious and platitudinous philosophizing" where formalism dominated literature and even music and where medicine abounded in "theorists and system-makers." Each of these characterizations has merit. Taken together they evoke a more accurate concept of the century.

In this book Dr. King offers a less positive and a much briefer evaluation. To him it "represents, perhaps, the adolescence of modern medicine." And unlike most medico-historical authors, he does not dwell on the major figures and the great achievements of his period. Rather, he lets the century speak for itself, not always through the great doctors but more frequently through his skillful presentation of the medical scene in its broader aspects. This is not a medical history of the century; it is, instead, a series of loosely related essays dealing with phases of the medical scene that are generally neglected in the formal histories. As such it presents to us a more vivid and graphic account of actual medical life than we are accustomed to reading.

In the picture of human health—in addition to the physician—the apothecary, the quack, the empiric, and the homeopath exert a decided influence. Here they are treated with as much emphasis as the doctor, medical ethics, the development of modern pathology, and the practice of medicine itself. In the chapters dealing with the latter subjects many of the leading physicians and scientists cross the pages of the book, but only incidentally. One figure is singled out for individual presentation: Boerhaave of Leiden, doubtless the most influential physician of his century. Widely renowned as a medical author and clinician and unsurpassed as a teacher, Boerhaave was known to his contemporaries as the *communis totius Europae praeceptor*. Inspired by him, his students carried his teachings and his plannings to the medical faculties of many universities, thereby helping them to attain lasting fame. The Scotchman, Alexander Monro *primus* returned from Leiden and laid the foundation of the importance of Edinburgh as a medical center; the Swiss genius, Albrecht von Haller, one of the greatest physiologists of all times, modeled the medical faculty of the University of Göttingen along the pattern developed by Boerhaave in Leiden; and the University of Vienna, previously dormant in the field of medicine, attained its eminence through two of Boerhaave's Dutch pupils, Gerhard van Swieten and Anton de Haen.

The question might be raised why the author has chosen this particular period for study. In addition to the considerations previously mentioned, it should be pointed out that medicine in the eighteenth century had achieved a complexity unknown in earlier centuries, and yet had not reached the diffuseness of structure of the nineteenth, which lends itself less well to this type of characterization. It was the period when the wisdom of antiquity was still part of the physicians' intellectual background and, at the same time, when the many innovations that were generated during the hundred-year span were eagerly accepted by the medical world. It was in this century that the important discoveries of the seventeenth century attained the recognition they merited. There was great delay in the practical application of Harvey's discovery of the circulation of the blood in 1628 and in the revelations of the early microscopists. The epidemiological experiences gained during the London plague and the work of the newly founded scientific societies hardly benefited contemporary medicine. Even Sydenham's clinical wisdom was not immediately appreciated by his contemporaries. In the course of the eighteenth century, however, all these achievements became part of medical thinking and furnished the basis for clinical practice and further scientific speculation.

Dr. King has brought to his study a particular empathy, which is founded on his own intellectual endowment. As in the period of which he writes, we find in him a combination of philosopher and scientist. With the clarity of his thought and his fortunate literary style he has created a book which is not only enlightening as a historical document but also a pleasure to read.

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Preface

Were we to believe everything we read about medical science and medical practice, we might think that we now live in an age of miracles presided over by supermen in white. So accustomed are we to the alleged wonders of present-day medicine that we tend to lose historical perspective. Hence, when we do look over our shoulder at the bygone days, it is very tempting to stress the great differences between then and now, to mutter a word of pity that our ancestors were so benighted, and to thank our own good fortune that we live in a modern, enlightened era. This attitude, however, is not at all necessary. Some of us, when looking backward, prefer to note similarities rather than differences. When we do so, we find that in the eighteenth century doctors were just as clever as they are today, just as perceptive and clear-thinking, exerting powers of observation and inference just as keen.

At the same time, eighteenth-century physicians were quite as muddleheaded, obtuse, grasping, prejudiced, and contentious as their descendants. In brief, those who lived two and a half centuries ago—and we are concerned particularly with doctors—differed in no essential feature from those who live today.

Of course, in the eighteenth century the total environment—the material factors and the entire conceptual framework—was so different from the present that basic similarities are disguised. Yet if we look sympathetically we readily discern those similarities and universal features which link together separate historical eras.

To induce sympathy we must try to capture the spirit of the past. There is more than one way of achieving this. The novelist, for example, may succeed brilliantly. William Makepeace Thackeray, comparing history and fiction in his *English Humourists of the Eighteenth Century* ("Richard Steele"), expressed this well: "I take up a volume of Dr. Smollett," he declared, "or a volume of *The Spectator,* and say the fiction carries a greater amount of truth in solution than the volume which purports to be all true. Out of the fictitious book I get the expression of the life of the time; of the manners, of the movement, the dress, the pleasures, the laughter, the ridicules of society, —the old times live again, and I travel in the old country of England. Can the heaviest historian do more for me?"

Sound words, well expressed. But a novel, even a historical novel, is, after all, fiction. And history, by definition, is something not fiction. History deals with *facts* and *truth,* those blessed words which have provoked interminable dispute. Thackeray, in the above quotation, raised the crucial problem, What is historical truth? To what extent, we may properly ask, is a novel true? What is the relation of the novelist or fiction-writer to the historian? To what extent is the sober learned essay true? Must historical truth be defined broadly or narrowly? These important questions face everyone who writes about the past.

To express my own philosophy of history I would offer an extended figure of speech. Quite familiar is the comparison between historian and painter. Both start with a "reality," something objective "out there" which, for the artist, is all simultaneous, for the historian, sequential. Yet both face an objective reality which they wish to depict. To what extent do they work similarly?

When we see a picture of a landscape, for example, we may wonder about many things. Why does the painter choose what he does? What does he leave out, and why does he leave it out? How closely does the finished work approach "reality"? How "accurate" is it? And even more important, how much aesthetic vitality does his picture embody? Has he painted a *good* picture? Without trying to answer these questions, we all recognize that a painter is quite deliberate in what he selects. What he sees is a result of his own personal insight, private and individual, which, through his chosen medium, he tries to convey to others. His rendition may be meticulously representational or it may show varying degrees of abstraction and distortion. But whatever the style or technique, the finished picture is the way *he* sees things.

If we compare a finished canvas with the scene supposedly depicted, we may note that certain details are omitted, others changed or distorted, still others simply invented. The literal-minded onlooker may therefore say the picture is not "true." To the artist this is a quite meaningless comment. He has sought a total effect, embracing a deeper reality that forms an artistic whole and conveys artistic truth. He has not sought a literal rendition. For that, he may say, you can take a photograph.

It is quite suitable, I believe, to compare such a painter to a historical novelist, and the "truth" of a painting—whatever that may be—to the "truth" of a historical novel. Both capture the spirit, both present an artistic whole, both are independent of literal detail. However, as we acknowledge our ties to literal detail, we approach closer to sober history. The modern historian may neither invent nor distort. He may not adopt the painter's license. He faces an objective reality which is not pliant. We may, perhaps, appropriately compare the historian not to the painter but to the *photographer.*

To many people the photograph is the ultimate of faithful reproduction, recording *facts* which are actually *there.* If only this were so, we could say the historian's function was to describe facts in photographic fashion, as accurately as possible. Such a concept, however, stems from a misunderstanding of both photography and history.

A neophyte with a new camera may roam the streets, point his camera hither and yon, and enthusiastically click the shutter. In a certain sense the result will be a literal transcript of what was "out there." Such pictures, nevertheless, are all too often poorly executed technically. They may be dull and uninteresting in subject matter, lacking any general appeal, any artistic value. When, on the other hand, we regard the works of expert photographers, we experience a quite different reaction. Perhaps it is a river or a meadow that is photographed, a patriarchal old man, an old fort or a herd of sheep, children at play or a building under construction. Whatever the subject matter, we appreciate a certain impact, a certain universality, timeless and real. This we feel in good photography. We miss it in the snapshot of Cousin Daisy posed before the Leaning Tower.

Like good painting, the good photograph embodies an artist's insight. There is the same carefully chosen point of view and admirable composition, the same range of tonal values appropriate to the subject depicted, the same inner harmony of parts. The photographer (like the historian) cannot invent, but he can carefully scrutinize the world about him and with an artist's eye select the exact view which to him is significant.

The photographer (and the historian) exerts innumerable subjective choices when he depicts reality. There is no such thing as *the* objective record independent of the recorder. Or, if there is, we can neither know it nor describe it. A photograph is always taken by a photographer—and history is written by a historian. The finished result will reflect what the historian or photographer wishes to reflect. A photographic print depends on a wide range of variables involving lenses, films, developers, and technical manipulations. Variation in these factors will mightily change the finished product; yet we cannot say that one result is more *real* or *true* than the other. One photographer might accentuate certain highlights or emphasize certain shadows. Is the finished picture therefore any less true? Such a question is not meaningful. The photographer, in accordance with his insight, perceives a certain limited totality. To express this, he aims at certain effects, achieved through technical devices. Another photographer, envisaging a different totality, would use quite other techniques. And still another, with no special insight or vision, might simply point his camera and shoot. Yet in so doing he necessarily uses certain technical procedures and automatically excludes alternative techniques. In other words, since any photograph requires some technical activity, the wise photographer adapts technique to his initial insight. And as no two photographers see things alike, so no two historical writers will present the same point of view, the same emphasis, the same inner vision.

All too much medical history has taken the form of the simple chronicle—dates, births, deaths, major achievements, and discoveries—quite bald and uninteresting. Such chronicles would, to continue our figure of speech, compare to the casual travel-snapshots that are fine mementos of a trip but are not very interesting to an outsider. The medical history to which I was exposed in my youth was, in essence, a series of indifferent snapshots of times past. We medical students were indeed a captive audience, extraordinarily unimpressed by the medical chronicle. Our attitude was admirably summed up in the then-current phrase, "So what?" Why bother? Unable to find an answer to these questions, we relegated the entire subject to our unconscious.

Yet, as the callow student slowly and painfully matures, he realizes that he cannot understand the present unless he understands the past. History is not remote. It is the living tissues of our present existence. The same forces that were active in the past are still at work now, expressing themselves in modern garb. Without knowledge of the past we cannot intelligently approach present-day medicine any more than a physician can intelligently diagnose or treat a patient without a sound past history of that patient. At any given moment a person is the summation of his entire past. To be sure, in any given context some facets of the past are more important than others, and what aspect of the past is most relevant to a given immediacy cannot always be readily selected. Precisely here is the physician's role so crucial. He must search out the particular details of the past which are most significant for the patient's present state. And the medical historian, like the physician, must seek out those past events, connections, meanings, and background which enter significantly into the present, as parts of its living fabric.

The eighteenth century represents, perhaps, the adolescence of present-day medicine. We will understand our mature present-day medical structure only if we appreciate its history. With this belief I have, so to speak, pointed my camera backward toward the past. Utilizing whatever insight and technique I possess, I reproduce a few scenes, trying to capture certain facets which appear to me both interesting and significant.

L. S. K.

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I

Apothecary and Physician

English medicine reached perhaps its lowest ebb in the early sixteenth century. The control previously exercised by the church had relaxed, and no other guiding force had furnished any replacement. Who was to practice medicine? Who was to supervise the practitioners? We do not know whether the public at large took much interest in these questions, but there definitely was expanding pressure from the available physicians to improve the situation. In the year 1511 King Henry VIII took steps to make a change. First, he lamented the general situation—and who actually wrote the words we do not know:

> Forasmuch as the science and cunning of Physick and Surgery . . . is daily within this Realm exercised by a great multitude of ignorant persons of whom the greater part have no manner of insight in the same, nor any other kind of learning:

> Some also can no letters on the Book, so far forth that common artificers, as Smiths, Weavers, and Women boldly and accustomably take upon them great Cures, and things of great difficulty; in which they partly use Sorcery and Witchcraft, partly apply such Medicines unto the disease, as be very noious [*sic*] and nothing meet therefore; to the high displeasure of God, great infamy to the Faculty, and the grievous hurt, damage, and destruction of many of the King's Liege people; most especially of them that cannot discern the uncunning from the cunning.[1]

This obviously was a very bad state of affairs. Having thus lamented, good King Henry decreed as remedy that to practice medicine within the city of London or for seven miles roundabout there must be an examination by the Bishop of London or the Dean of St. Paul's, who would call upon four doctors of "Physick," and also on expert surgeons, to assist him. No one was to be allowed to practice, under penalty of £5 a month fine, unless he had been so examined. Outside of London, the examination was to be conducted by the bishop of the diocese. And finally—a matter extremely significant for all later development of medical practice—it was explicitly stated that nothing in the act should be construed as prejudicial to Oxford or Cambridge universities.

This regulation apparently failed of effect, for later, by acts of Henry VIII in 1522–23, a new system was decreed. In popular estimation Thomas Linacre received the credit for these new decrees which established a College of Physicians. In the act, six physicians including Linacre were expressly named as constituting a college, "in re & nomine unum Corpus & comunitas perpetua sive Collegium perpetuum,"[2] which had specific powers, rights, and privileges: The college could own property, conduct assemblies, have a seal; it could sue and be sued, could have certain specified offices, and, most important for our purposes, it held a monopoly on the practice of medicine. No one could practice within the city of London or its environs, unless he were admitted by the president and fellows of the college. It was expected that no one should practice medicine "but oonly these persons that be profound sad and discreete groundly learned and deeply studied in Physike."[3] Elsewhere in England no one could practice physic unless he had been examined in London by the president and three "Electys" (who corresponded to censors or examiners).

Now there were two important provisos. The original incorporation not only mentioned six doctors expressly but also included the phrase *Ipsi omnesque homines ejusdem facultatis de & in civitate,*[4] meaning, "they and all men of the same faculty of and in the city." This referred to graduates of Oxford and Cambridge, who thus had and were to continue to have special privileges of practicing medicine in London. Furthermore, it was expressly stated that outside the capital graduates of Oxford and Cambridge were exempted from required examinations and could practice medicine without submitting to the forms required of others.

The College of Physicians received further powers. Not only could it regulate the practitioners of medicine but it could also—in theory—control the dispensing of drugs. That is, by an act of Henry VIII in 1540, the college had the right to choose four physicians who would have full authority "to enter into the house or houses of all and every Apothecary . . . to search, view, and see such Apothecary-wares, drugs and stuffs, as the said Apothecaries . . . shall have in their house" and then, after calling the wardens of the apothecary's guild, could destroy any defective or corrupt drugs.[5] These powers, subsequently renewed and redefined, made the college a very powerful body.

But the physicians were not the only ones to organize. We remember, of course, that the Renaissance physicians descended from the ecclesiastical practitioner who, hemmed in by church law, could not shed blood. The menial tasks, like performing surgical operations and compounding medicines, were beneath the dignity of the doctor. Although he could direct, he left the performance to others. Surgery was the province of the barbers or barber-surgeons, while apothecaries prepared and compounded drugs. The breakup of medieval institutions seriously affected the physicians, since the "great multitude of ignorant persons," mentioned above usurped his prerogatives and forced him to create a guild in self-protection. Other types of medical practitioners—the surgeons and the apothecaries—also formed their own organizations.

The barbers guild antedated that of the physicians, for in 1462 Edward IV granted a royal charter to the barbers authorizing them to practice surgery. This was not a monopoly, for an unincorporated guild of surgeons also existed. It was in 1540 that the guild of surgeons officially joined the Company of Barbers, the conjunction remaining in force until 1745, when surgeons attained independent corporate status.

The principal discords concerned the apothecaries. In 1606 they joined into a single company with the grocers, but in 1617 they founded a separate Society of Apothecaries. The charter granted by James I named 114 original members. The society constituted a "body politick" capable of purchasing land, having a seal, owning a hall. The society could sue and be sued, could make laws and levy fines. It could choose its own master and wardens. No grocer, under penalty, could keep an apothecary's shop. No one was to keep a shop until he had served a seven-year apprenticeship, and no apprentice was to be set free without examination. The society had the power to examine drugs and condemn those which were unsound, but the charter explicitly declared that there was to be no prejudice to the powers of the College of Physicians.[6]

The apothecary was, as the etymology indicates, originally a shopkeeper. He was in essence a grocer who dealt with a rather specialized product and, as a specialist, felt the need to break away from the parent body. His own particular trade had special problems and special duties not shared by other shopkeepers. The apothecary, who compounded and sold the remedies that the physician ordered, had a primitive code of ethics, not formally binding but illustrative of early ideals. Among other regulations, his shop must be clean, he must know the natures of the plants and herbs, he must fill the physician's "bill" (prescription) exactly, not making any substitutes, omissions, or additions. He should neither buy nor sell rotten drugs and must use true weights and measures. These exhortations, obviously, imply that the ideal was rarely met. The apothecary was permitted to "open wel a vein for to helpe pleurisy." But the most significant precepts were "that he meddle only in his vocation" and "that he do remember his office is only to be ye phisician's cooke."[7]

Here is the crux of the whole subsequent disturbance: the apothecary was originally the "phisician's cooke," who carried out orders. A good apothecary carried out his orders well and faithfully, and this, apart from minor bloodletting, was his sole function.

The physician wished to maintain this relationship *in statu quo*; the apothecary wished to alter it. Any organized group has a certain growth potential, an impulse to expand, increase in importance, and overcome any hostile limiting environment. The struggle between physicians and apothecaries, which helped change the entire practice of medicine, illustrates what happens when there are two competing groups, each with an impulse to expand.

It would not be profitable to trace the early details of the struggle. Omitting the earlier quarrels, we can skip to the end of the seventeenth century and the beginning of the eighteenth, when a definite pattern already was well established: the apothecaries were practicing medicine, albeit they were not physicians and did not have any license. This state of affairs came about gradually, with very many different factors operative. There had not been enough physicians, and, because of the too great demand upon them, they had "to dismember their Act into three Parts, whereof two were servile, Chirurgery and Pharmacy."[8]

By having others do part of their work, physicians, "by the Advantage of their Chirurgeons and Apothecaries, were capacitated to visit and cure ten times greater numbers of sick then before; which in a short time improved their Fame and Estate, to a vast Treasure." Here we have all the elements of an exploitation. So long as the individual physician did all his own work, compounding and dispensing his own drugs, no one begrudged him whatever prosperity he might achieve. But when, by utilizing the labor of others, he vastly multiplied his own income, those others, whose work maintained his prosperity, might justifiably feel restive. In short, the apothecaries, who helped the physicians so much, aspired to an equality of wealth. They felt that they could prescribe medicines as well as prepare them, and held that "those that had made and dispensed the Medicines were as able to apply them to the like Distempers, as they that had prescribed them."[9]

This is a debatable proposition, but, whether we agree with it or not, it does express an honest, if limited, opinion.

The apothecary, if he noted what a physician prescribed for one patient, could then advise the same medicine in any similar case. There are certain assumptions here: first, that what the physicians prescribed was appropriate and suitable to begin with; and, second, that the apothecary could recognize a similarity of cases. If we grant these assumptions, then the apothecary stood on reasonably firm scientific, if not legal, grounds. We have a present-day counterpart to this situation when a man goes not to a doctor but to a neighborhood drugstore and asks the clerk for a cough remedy or for something to cure rheumatism or headache. The clerk, knowing that doctors prescribe certain medicines, can recommend the same preparation, assuming that what is good for one man's cough would be equally good for another's. This, as a general logical assumption, is quite fallacious, even though in isolated cases it might prove valid.

We must not think that this fallacy is visible only to our twentieth-century eyes. It was equally obvious in the eighteenth century. Lacking expert knowledge, the apothecary could only guess whether a given remedy were applicable. The guess was often wrong, based on imperfect analogy and misleading resemblances. Those who were learned in medicine would find that whereas "the Physician gives very high Cordials in the cold Sweats—the other gives the same the next day in the burning Paroxysm. . . . The Physician blisters the Stupid and Lethargic: He blisters the Wakeful and the Raving."[10]

Today we might ask, in retrospect, What difference did it make? This is not a fair question, since it completely ignores the context and the historical perspective. The fact remains that, in the process of carrying out the orders of others, those who are perceptive can learn to give orders themselves. The apothecary needed only to keep his eyes open in order to learn medical practice from the prescriptions which the physicians wrote. In brief, as Alexander Pope declared, in his pithy style:

> . . . modern 'pothecaries taught the art
> By doctor's bills to play the doctor's part.
> Bold in the practice of mistaken rules,
> Prescribe, apply, and call their masters fools.[11]

The physicians did not take kindly to this and even made some abortive attempts to frustrate the apothecaries. At the end of the seventeenth century, at the time of the dispensary dispute described below, the College of Physicians decided that the Latin prescriptions sent to the apothecaries should contain merely the ingredients, not the directions for use. How the medicine was to be taken or how applied was to be given in English to the patient or sent directly to the patient's home. In this way the physicians thought the proper use of medicines would not reach the apothecaries.[12] There is no indication that this move was in any way effective, but it does indicate extraordinary pettiness and bad judgment on the part of the physicians.

The apothecaries did not have a completely free hand. They might be prosecuted by the College of Physicians for practicing medicine illegally, a danger leading to a *cause célèbre* which we will discuss shortly. But they also ran the risk of committing felony, for we learn that "by the Law of England, if one who is no Physician or Surgeon, being not expressly allowed to practice, shall take upon himself a Cure, and the patient die under his Hands, this is a Felony in the person presuming so to do."[13] Consequently, it was the custom among apothecaries, when the patient appeared dangerously ill, to call a physician who would assume responsibility. The apothecary felt competent to prescribe for sick patients, but, if there was no adequate improvement, he would call a consultant to take over.

Here, then, is a situation fraught with explosive possibilities. Whom should the apothecary call? Ordinarily there was considerable choice. Why select one rather than another? If we presuppose a rather high ethical standard wherein the patient's welfare comes first, the apothecary would call the physicians with the greatest reputations, who would give the patient the best attention. But there is evidence that venal motives played a very large part. The apothecary might put his own welfare first and call as consultants those who best "co-operated." Declared one contemporary source, the apothecary "hath the power of the Keys committeed to him." He could pretty much call or exclude whom he pleased, and quite naturally he would recommend only those "who will give the highest Encomiums of his [the apothecary's] Skill and Judgement, and justify the use of many Doses."[14] That is, the consultant was expected to praise the apothecary to the family and also to order many and complicated prescriptions which the apothecary would fill. There was no direct fee-splitting. Instead the consultant paid off a little obliquely, through having the patient buy large quantities of medicine from the apothecary. Boluses, juleps, clysters, electuaries, plasters, ointments, and pills served not only to dose the patient but to support the apothecary in comfort.

Those physicians who did not order largely were not so likely to be called again in consultations. Nor would they be called again if they were too critical of the way the case had been handled up to that point. They might indeed feel quite aghast at the treatment, but it was very poor policy to say so publicly. Then as now, the consultant might grumble because he had not been called earlier. He might also be distressed at the very considerable charges already borne by the patient, money which, he might feel, was badly misspent. But unless he was very independent he ordinarily said little.

It is important to realize that the apothecary, not being licensed to practice medicine, could not charge for advice. He could charge only for the medicine he furnished. The abuses that this system made possible are indeed staggering. Take an ignoramus not quite sure what he is doing, then allow him to furnish one medicine after another in the hope of achieving a cure, and the total charges can pyramid alarmingly. Everitt records a case at the end of the seventeenth century in which an apothecary, treating a corn, applied caustics which produced an ulcer that utimately penetrated to the bone. Before the patient was finally cured, eleven weeks had elapsed, during which time the apothecary furnished medicines at a total charge of £132 12 *s.* 8*d*. The bill was finally compromised for £100. The actual cost of the medicines was less than £7, yielding a rather tidy profit.[15]

The bills for medicines mounted rapidly. In the early eighteenth century apothecaries charged half a crown or three shillings each for clysters, plasters, cordials, or vesicatories. In general, medicines ranged from one to three shillings for each item. The apothecary, were he taking full care of the patient, making daily or twice-daily visits, might very easily run charges up to fifteen shillings a day, and then sometimes the patient might offer a gratuity—a free-will offering and not in any sense a professional fee. Ordinarily the patient would recover, which would enhance the apothecary's reputation. If the patient got worse, the apothecary would then call in a physician as consultant. The doctor, who legitimately charged for his opinion—usually a guinea per visit—would ordinarily order further medicines, giving further business to the apothecary. The latter thus had a double source of income: some came from his own efforts, but, if his own efforts failed, further income would ensue from the consultant's opinion.

That there was a considerable spread between the prices charged for drugs and the actual cost of the drugs aroused much resentment among physicians. The markup was, of course, simply a means of giving the apothecary a professional fee while maintaining the illusion that the advice was free. The apothecary "makes the deluded Patient pay very extravagant Fees by the intolerable Prices he puts on all the cheap Medicines, and by passing upon him very many more Doses, than the Disease requires or the Constitution can bear."[16] Dazzled by the thought that they paid no fee, the patients blindly accepted all the medicines the apothecary might order and rarely argued about the large quantity or the high prices. But they would grumble at the thought of paying a physician—a doctor of medicine—for his advice.

A grasping apothecary had many disreputable ways to enrich himself. He could use, or claim to have used, various exotic ingredients of supposed medicinal virtue, such as bezoar or pearl or gold or precious stones, not to mention powdered mummy, powdered viper, horn of rhinoceros, molted skin of reptiles, and the like. These ingredients, supposedly rare, costly, and correspondingly effective, were by the enlightened recognized as worthless and cheap. Nevertheless, their use would help excuse high charges. Furthermore, the unscrupulous apothecary could modify the doctor's prescription as he chose, using whatever drugs he might happen to have on hand rather than filling the prescriptions accurately. Ordinarily no one would be the wiser. Or, while supplying the actual items called for, he could use old "worm-eaten superannuated Drugs"[17] rather than fresh, active ingredients.

But we must not think that the apothecaries were necessarily the villains of the field, while the doctors were the heroes. There was one simple but stubborn and inescapable fact—physicians, with their long, costly education, limited numbers, and high fees, could not possibly satisfy all the medical needs of the community. There simply were not enough doctors to go around, and their services were too expensive, giving rise to the gibe that the physician was like Balaam's ass because he would not speak until he saw an angel (archaic coin worth about ten shillings). But the poor nevertheless required medical care, and the well-to-do, when their ailments were not too severe, wanted medical attention more often than the physicians could respond. There was indeed a medical vacuum, which the apothecaries rushed in to fill. But, with regard to prestige, they were definitely second-class citizens, constantly trying to improve their financial and social status and often trying genuinely to improve their professional competence.

The relationship between physicians and apothecaries was quite checkered—sometimes there was co-operation, sometimes competition and resentment. The co-operation was often on a very high level, where each had the welfare of the patient at heart. The good apothecary would realize his limitations and would always send for a physician-consultant when trouble threatened. And he would co-operate fully and honorably with the consultant. The physician, in turn, appreciating the services the apothecary rendered, would carefully maintain the latter's prestige with the patient. Each recognized the relationship of symbiosis. On the other hand, when genuine co-operation failed, when competition, jealousy, and rivalry supervened, each would try to tear down the other's reputation and standing.

At the end of the seventeenth century the enmity between physicians and apothecaries was especially violent.

One famous case has become part not only of medical history but of literary history as well. It all had to do with medical care for the indigent. In 1687, before any adequate hospital systems were established, the College of Physicians decided to render free medical service to the sick poor. This, as a humanitarian move, was not quite as helpful as might at first appear, since the doctors, treating the medically indigent, would usually give them prescriptions. These the poor patients took to apothecaries but lacked the means to pay for the medicines. The apothecaries were not a party to the charitable enterprise. The physicians felt that the apothecaries should contribute to the care of the indigent by filling prescriptions at cost. As might be expected, there was little enthusiasm for this suggestion among the apothecaries, even though a few complied. The physicians declared that the response was not adequate. It was therefore decided, by many of the physicians, that the college should take over the apothecary's functions and that henceforth the college would furnish not only advice but drugs as well, the latter to be sold at cost.

There was the following resolution:

> Whereas the several Orders of the College of Physicians, London, for prescribing Medicine *Gratis* to the Poor Sick . . . as the Proposals made have hitherto been ineffectual, for that no Method hath been taken to furnish the Poor with Medicine for their Cure at low and reasonable Rates; we therefore whose Names are here under-written . . . being willing to promote so great a Charity . . . do oblige ourselves to pay . . . the Sum of Ten Pounds a piece of Lawful Money . . . which Money when received . . . is to be . . . expended in preparing and delivering Medicine to the Poor at their intrinsic value. . . .[18]

Fifty-three members subscribed to this instrument, and the college went into the business of filling prescriptions.

The step appears logical to improve the quality of medical care furnished the indigent. But it was also a move in professional power-politics to compete with the apothecaries, regulate their practices, and, in modern terms, to furnish a yardstick to measure and control their prices. Naturally there was resentment at this allegedly unwarranted interference. The motives behind the new policy were impugned. Although it was maintained that no profit was involved, the skeptics would not believe it. The apothecaries felt that it was a move to undersell and ruin them, and there was probably some truth in the allegation. All physicians did not agree on the wisdom of the step. Many opposed the dispensary, for they did not want to antagonize the apothecaries for whom they were consultants. But the college, despite the opposition, went right ahead, setting aside a room in which the prescriptions would be prepared. This room was in the regular college building in Warwick Lane.

The conflict between physicians and apothecaries found expression in a vast polemic literature, often scurrilous and abusive. The one contemporary work still remembered is Samuel Garth's long poem, *The Dispensary*. Garth was a prominent physician and not a bad poet. His satire, which won wide approval and went through numerous editions, is worth more than passing mention. The poem, in classical rhyming couplets, takes an allegorical form. Loaded with mythological and classical allusions and contemporary references, it is at first a little rugged for the modern taste. But after one or two readings the difficulties recede, and the modern reader can appreciate the rollicking spirit and clever satire, especially if he ignores the more obscure references. The story in brief:

The College of Physicians, originally an active seat of learning, is now hushed. The God of Sloth dwells there, spreading quiet laziness and apathy where once reigned vigorous scientific investigation. But Sloth is aroused by a sudden frenzied commotion—workmen are reconstructing the building to make the dispensary. Disturbed and troubled, the God of Sloth wishes to silence the din and restore his reign of laziness. To obstruct the projected building, he seeks allies among those opposed to a dispensary. We are introduced to various apothecaries and quacks, as well as to various dissenting physicians who, although members of the college, oppose the majority opinion and side with the apothecaries. There is "Colon," satirizing a Mr. Lee, warden of the Society of Apothecaries, who is "In Morals loose, but most precise in Look." There is a quack, designated "Horoscope," representing a Dr. Bernard, once an apothecary, given to fortune-telling and predicting the future as well as to prescribing physic: he reminds us of the Dr. Partridge demolished by Jonathan Swift. Garth described Horoscope's shop:

> Here, *Mummies* lay most reverendly stale,
> And there, the *Tortois* hung her Coat o'Mail
>
> . . . .
>
> Aloft in Rows large Poppy Heads were strung.
> And near, a scaly Alligator hung.
> In this place, Drugs in musty Heaps decay'd,
> In that, dry'd Bladders and drawn Teeth were laid.

Colon describes the threats which emanate from the College of Physicians and the unfair competition in dispensing profitable drugs. He laments:

> What if We claim their Right t'Assassinate,
> Must they needs turn *Apothecaries* strait?
>
> . . . .
>
> Our Manufacture now they meanly [cheaply] sell,
> And spightfully th'intrinsick Value tell:
> Nay more: Inhumanly They'll force us soon
> T'exert our Charity, and be undone.

To defend themselves the apothecaries, after various meditations and incantations, meet to decide a course of action. They enlist the aid of numerous physicians who oppose the dispensary. There is the outstanding champion "Mirmillo," representing Dr. Gibbon, who sides with the apothecaries, enjoys great business from them, and wants to preserve the current systems and help repay the apothecaries for the many fees received. He boasts of his own powers:

> And you'll perceive, this Hand, when Glory calls,
> Can brandish Arms as well as Urinals.
>
> . . . .
>
> Whilst others meanly ask'd whole Months to slay,
> I oft dispatch'd the Patient in a Day:
> Some fell by *Laudanum,* and some by *Steel,*
> And all the feather'd Folks forbear their Lays of love.

Numerous other physicians, including such pseudonyms as "Querpo" and "Carus," oppose the Dispensarians. The poem presents considerable discussion with many topical allusions which are lost on any modern reader who is not a minute scholar of the period. Interspersed, however, are occasional flashes of genuine poetic feeling. For example, the following lines, descriptive of evening, compare favorably with any eighteenth-century nature poetry prior to Goldsmith's "The Deserted Village":

> The Clouds aloft with golden Edgings glow,
> And falling Dews refresh the Flow'rs below.
> The Bat with sooty Wings flits thro' the Grove,
> The Reeds scarce rustle, nor the Aspine move,
> And all the feather'd Folks forbear their Lays of love.

But satire does not yield to lyric poetry. Garth continues with biting couplets. Not without much inner struggle, the dissenting doctors decide to fight. They have weapons. One champion, Querpo, has a shield, handsomely embossed, displaying

. . . *Leeches* spouting *Hemorrhoidal* Blood.
The Artist too express'd the solemn State
Of grave *Physicians* at a Consult met;
About each Symptom how they disagree,
But how unanimous in case of Fee.
Whilst one *Assassin* his learn'd Collegue tires
With quaint Impertinence, the Sick expires.

The actual combat between the dispensary physicians and their opponents has amusing touches:

The piercing *Causticks* ply their spightful Pow'r;
Emeticks ranch, and Keen *Catharticks* scour.
The deadly Drugs in double Doses fly;
And Pestles peal a martial Symphony.

. . . .

From *Stentor's* arm a massy Opiat flyes,
And strait a deadly Sleep clos'd *Carus'* Eyes.
At *Colon* great *Sertorius* Rhubarb flung,
Who with fierce Gripes, like those of Death, was stung;
But with a dauntless and disdainful Mien
Hurl'd back Steel Pills, and hit him on the Spleen.

. . . .

Each Combatant his Adversary mauls
With batter'd *Bed-Pans* and stav'd *Urinals.*

Querpo and Stentor fight. The latter, triumphant, is just about to puncture his enemy with a lancet, when "*Apollo* interpos'd in form of Fee" and the victor withdrew his arm. The prospect of a fee thus ends the battle.

The final canto is somewhat of an anticlimax. Celsus, the physician, makes a trip to the Elysian Fields and the nether world. In the course of his visits he addresses the goddess Hygeia:

With just Resentments and Contempt you see
The mean Dissentions of the Faculty;
How your sad sick'ning Art now hangs her Head,
And once a Science, is become a Trade.

Her Sons ne'er rifle her Mysterious Store,
But study Nature less; and Lucre more.

These verses indeed express the early eighteenth-century attitude.

The revolt was in vain. The dispensary, firmly established, provided medicines and prescriptions to the indigent at cost, or so at least its supporters claimed. To be sure, it was admitted on all sides that the well-to-do also frequently obtained their medicines from the college at marked savings, but this, although it bothered the apothecaries and seemed like unfair competition, did not disturb the college members. At any rate it is said that the college dispensary made up about twenty thousand prescriptions yearly wherein the doses of electuaries, juleps, pills, and the like came to about a penny apiece.[19]

The apothecaries, however, continued to flourish and increase. At the beginning of the eighteenth century their number in London was estimated at about 1,000. At the same time the College of Physicians included 66 fellows, 8 candidates, 4 honorary candidates, and 36 licentiates, 114 in all, of which total it was estimated that only 60 to 80 practiced in London.[20] Quite obviously, if the apothecaries restricted themselves merely to filling prescriptions and selling drugs, the profession had seven or eight times as many as were needed. But, instead of keeping shop, they actively practiced medicine, despite legislation and prosecution by the physicians. The conflict between the two groups reached its decisive climax in 1703–4.

The Royal College of Physicians, ever since its inception, had prosecuted quacks and others who practiced medicine in defiance of the monopoly granted the college. The prosecution of an apothecary named Rose was not in itself unusual, but the final decision was epoch-making. The bare facts are simple. A butcher named Seal consulted Rose for treatment. The latter, when his bill was not paid, had the butcher arrested. Apparently the patient enlisted the aid of the College of Physicians, which brought Rose to court for what modern terminology would call practicing without a license. Rose was convicted at the Court of Queen's Bench. The law was clear. The court held that only the physician could judge the nature of the disease, choose the remedy, and order its application. The proper function of the apothecary was to make up the prescriptions of the doctor, according to the directions. It was said that the sympathies of the jury lay with the apothecary, but the charge from the bench was unequivocal. The law, whether it was good or bad, was clear, and the judges were unanimous. Rose, however, appealed the verdict to the House of Lords, which, acting on equity rather than on the letter of the law, reversed the decisions. The grounds for reversal were that public need as well as custom required that apothecaries be allowed to advise patients, that the monopoly held by the college kept down the number of physicians, and that it would mean undue hardship to deny the public all other sources of medical aid.

Additional data cast further light on this simple recital of facts. When the case was appealed to the House of Lords on a writ of error, the opposing arguments were very illuminating. Both sides naturally tried to offer the best possible face. Both sides revealed wanton disregard for truth or simple equity, yet at the same time each side scored telling points against the other. In the end, what today seems to have been rather disgraceful chicanery yielded beneficial results two and a half centuries ago.

It is quite illuminating to consider a few of the points. The counsel for the College of Physicians introduced the following signed complaint from the butcher, John Seal:

> These are to certify that I, John Seal, being sick, and applying myself to this Mr. Rose, the apothecary, for his directions and medicines in order for my cure, had his advice and medicines from him a year together; but was so far from being the better for them, that I was in a worse condition than when he first undertook me, and, after a very expensive bill of near 50 l., was forced to apply myself to the dispensary at the College of Physicians, where I received my cure in about six weeks' time, for under 40s. charge in medicines.[21]

It is worth noting that £50 was quite a huge fee; at the rates then current, it must have represented a vast array of medicines. Rose's attorney made no direct answer to the allegation but, in his general defense, made the following plea: that apothecaries should be allowed to sell a few simple medicines without being considered lawbreakers. He asked that "selling a few lozenges, or a small electuary, to any asking for a remedy for a cold, or in other ordinary or common cases, or where the medicine has known and certain effects, may not be deemed unlawful, or practicing as a physician, when no fee is taken or demanded for the same."[22] The apothecaries thus made themselves out to be entirely blameless and innocent, quite ignoring the highly vicious practices charged by Seal. Under no stretch of imagination could the patient be deemed to have requested £50 worth of medicines, nor could these medicines have possessed "known and certain effects," or else the distemper would have been cured. If we believe Seal, it appears that Rose was unquestionably guilty of practicing medicine without a license. To be sure, he got no fee for his advice as such, but the charges for medicines more than covered the seemingly gratuitous opinion. And, by the strict letter of the law, the apothecaries had no right to render opinions.

Rose's lawyer did not have much of a case, apart from sniping back at the physicians. The apothecaries, he pointed out, would be ruined unless the original judgment were reversed. The physicians, he indicated, kept a monopoly. They had also set up shop and, under the pretense of charity, were multiplying nostrums or secret medicines and selling them very dear. To permit this, and at the same time to forbid the apothecary to sell a few lozenges would be oppressive to the poor and prejudicial to the acutely sick who, when new symptoms suddenly arose, might not be able to find a physician.

The lawyer for the College of Physicians entered a rather lengthy plea, answering in detail the apothecary's allegations. He quite correctly derided the excuse of "selling a few lozenges," pointing out that apothecaries undertook any and all kinds of cures, even in the most difficult cases, shutting out the physicians until danger forced a consultation. And, if the apothecaries were forbidden so to treat, they would not be "ruined." They could go back to making, compounding, and selling good wholesome medicines according to prescriptions, becoming again related to the grocer from whom they stemmed. Then, showing the true colors of his clients, he declared of the apothecaries that it would be a "gross absurdity" if physicians "should desire to put 114 men, who were used before to attend them as their servants, upon the same foot and level with them."[23] Cogency of argument here gives way to inexcusable insolence. This is indeed the *hubris* of the Greek tragedians, the pride that betokens a disastrous fall.

Counsel admitted that physicians promoted nostrums or secret medicines, but he tried to make a positive virtue out of the practice. The excuse was that, unless the medicines were kept secret, the apothecaries would misuse them. The physicians were thus "under a necessity either of prostituting these (the medicines) to the abuse of the apothecaries, through adulteration, exaction, or presumptuous ill application (by which the best medicines are often brought into disgrace), as to keep them secret, and take from the apothecary a pretence of knowing all our practices, and thereby abusing the people under color of our authority."[24] This type of reasoning, of course, was scarcely calculated to foster good public relations.

Then the lawyer denied that the apothecary could plead urgency and suddenness of a case as an excuse for irregular practices. In a quite intolerable manner he declared that "certainly no persons . . . live so far from a physician as not to be able to call one in four-and-twenty hours time."[25] He said, in effect, that, if a patient could get a physician's attendance within twenty-four hours after desiring it, there was no ground for complaint and no reason to plead sudden or unexpected need. Or, in other words, he implied that the number of physicians was quite adequate to the medical needs of London, if twenty-four hours was accepted as a reasonable interval after a call. This, however, was so palpably contrary to fact that it could not stand. When a lie gets too big, it collapses of its own weight. Both the apothecaries and physicians uttered lies of this magnitude. Public policy clearly dictated that, however bad the apothecaries' practice might be on occasion, it was preferable to the rigid, supercilious monopoly of a few physicians. There clearly was evil on both sides, but the House of Lords chose a lesser evil when it reversed the original judgment. Thus, they permitted apothecaries to practice medicine without being, even in theory, merely the servants of the physician. This decision has been called the Magna Carta of the general practitioner, for it established the status of the apothecary and allowed him to transform into a primitive general practitioner.

The evils very gradually receded. As the apothecaries improved their status, the need for shady practices diminished. The physicians and apothecaries began to work together a little more closely. The famous Dr. Mead helped good relations very materially when he devised a very happy means of consultation. He would frequently spend part of the day at a coffee house. Apothecaries would repair thither, bringing verbal or written reports of their cases. Mead would listen, would study the reports, and then, without seeing the patient, would give advice—at a half-guinea fee per case. This was indeed an inspired solution. The patients got good advice even if it was at second hand. The physicians, by relatively small effort, materially increased their incomes and preserved their status as the higher authority. The apothecaries not only kept the physicians happy but broadened their own experience, probably rendered better service, and assumed increased stature in their patients' eyes. We might harbor a bare suspicion that the whole transaction, above and beyond legitimate consultations, savored just a little of "protection money" or subtle bribery. But the fact remains that, as apothecaries called on physicians more and more, opposition and conflict declined.

The transformation of the seventeenth-century apothecary into the modern practitioner was a long slow process, punctuated by many acts of Parliament. The details of medical legislation, defining the practice of medicine and setting appropriate standards, form a separate study which more properly belongs in a history of the nineteenth century.

One very important factor, however, belongs to our period: the increased educational opportunities for medical training, especially through the growth of Scottish universities. Better educational facilities permitted men who started life as apothecaries to become physicians. The two professions no longer were sharply sundered. When professional mobility became possible, when a man could move from the lower rank to the higher rank, it was only a matter of time until distinctions were obliterated. Although this time still lay considerably in the future, the groundwork was set in the early eighteenth century.

As we have seen, the universities of Oxford and Cambridge originally held a complete and rigid monopoly in the training of British physicians. The training was not very good, and the best physicians all studied in the Continental schools—particularly Leiden, although Paris, Montpellier, Bologna, Padua, and Rome also had much to offer. But the English universities, tied in as they were with the College of Physicians, held an unassailable position.

Unfortunately, the universities were too closely connected with politics and the religious struggles under which political enmity masqueraded. The Civil War of the seventeenth century, the restoration of the House of Stuart, the Bloodless Revolution of 1688 and the accession of the House of Orange, and then, in the early eighteenth century, the reign of Anne and the accession of the Hanover dynasty, all represented an epoch as thrilling as any the world has ever seen. Dynastic uncertainty, political chicanery, and religious intolerance affected all walks of life, the universities not the least. Ultraconservative, they long refused to change with the times. They admitted as students only those of sound orthodox faith, while dissenters could not even matriculate. Deprived thus of the stimulus which comes from dissent, the universities remained comfortably in an old, outmoded pattern in which political expediency replaced the spirit of learning and in which true intellectual ferments were sadly lacking.

Medical training was excessively long. To receive the degree of doctor of medicine at Oxford originally required fourteen years attendance at the university: four years for the bachelor of arts, three more for the master of arts, three more for the bachelor of medicine, and four more for the doctor of medicine.[26] Such a person had, of course, not only a professional but also a "liberal" education. For his bachelor's degree he had, in the best medieval style, to pass examinations in which he "defended" questions in logic, grammar, rhetoric, and moral philosophy, all, naturally, in Latin. For the master's degree, additional examinations included natural philosophy, geometry, metaphysics, optics, physics, and history. The medical education was largely a study of the ancient authors; the examinations for the doctor's degree required an explanation of a whole book of Galen in six extemporaneous lectures or three written lectures. To such an extent had this gone that Sydenham is said to have declared that medicine was not to be learned by going to universities. He favored taking apprentices and said that "one had as good send a man to Oxford to learn shoemaking as practicing physick."[27]

A bachelor of medicine from an English university could become a physician in the army or navy, even though his education did not progress much beyond the aphorisms of Hippocrates.[28] The "examination" consisted in defending two questions, one chosen by the candidate and one by his professor. But the latter question would be given him as long before the examination as he wished. No one, it is said, ever failed an examination for this degree. Candidates for the M.D. were sometimes rejected but rarely if ever on the grounds of intellectual achievement. Moral and political reasons, however, could lead to refusal of a degree.

Medical education at the English universities was indeed very inadequate. Lectures in physic and anatomy, botany and chemistry, were perfunctory, and there was no practical instruction. Hospital facilities were not available in the university towns. There was a fine classical education with some tincture of "phylosophy," that is, "natural philosophy" or science, but virtually no resurgent drive or progressive spirit and nothing that would bring medieval medicine into a modern vital practice adequate to contemporary needs.

It was therefore fortunate from the long-range view that the universities were both exclusive and expensive. Study abroad had always been available but only to the very, very few. A vigorous and determined youth had to find other forms of medical education. Foreign schools, although offering excellent training, were scarcely a satisfactory solution. The new demands brought forth new solutions along several different fronts. One was the development of hospitals, especially in London, where students could secure practical training in the wards. Since a green youth could not profitably learn medicine or surgery without a background, it became necessary to provide one. This took the form of private lectures, private schools, and private dissecting academies which together offered a thorough grounding in the "basic sciences."

A superb picture of the London student's life in the mid-eighteenth century may be found in *Diary of a Surgeon.*[29] Although fictitious, it is for the most part a very accurate rendition of contemporary material, well presented and eminently worth reading.

Apart from instruction connected with the hospitals and the development of private lectures and academies, there was also the rise of new medical schools, especially in Scotland. Of these the University of Edinburgh was outstanding. The town council of Edinburgh appointed three medical professors early in the century, and from this small beginning developed an institution which soon became the leading medical school in Great Britain; before the century was over, it was probably the best in the entire world. At first, instruction was quite informal with no regular curriculum. For example, in 1706 Sir Robert Sibbold, professor of medicine, advertised that he would teach a private class in medicine, but he stipulated that the applicants must be skilled in Latin and Greek, in philosophy (in other words, physics and chemistry), and in the fundamentals of mathematics.[30] These requirements were equivalent to a bachelor's degree in arts and indicated high standards.

Dr. Sibbold did indeed try in his early classes to establish suitable standards, but these proved quite impossible to maintain. The Scottish universities—Edinburgh, Aberdeen, Glasgow, St. Andrews—developed the practice of granting medical degrees *in absentia*. That is, matriculation, residence, and examinations were not required. Anyone could be a candidate for a degree who presented letters of recommendation from two physicians, offered a suitable thesis, and paid the necessary fees. Recommendatory letters were always easy to come by, and the exact authorship of a thesis never received too close scrutiny. As a result Scottish medical degrees were quite easy to obtain, and the Scottish universities became diploma mills. It was a very curious paradox that a degree from, say, the University of Edinburgh might represent very excellent training, as good as any in the world if the candidate had faithfully spent his time in residence; or it might represent no training at all but merely the possession of some complaisant friends, if the degree was bestowed *in absentia*.

More commonly, however, the M.D. degree was of intermediate significance. Many young men took varied training, including a certain amount of apprenticeship with an apothecary or a surgeon, a semester or two or three of university lectures, a variable course of study at a dissecting academy and a London hospital, taken in any combination and in any order; and then, after a long or short lapse of time, he might request and receive a degree of M.D. *in absentia* from one of the Scottish schools or from a European school after a very brief sojourn abroad. Many individuals started their professional careers as apothecaries but finally achieved a doctor's degree. Such men could practice as physicians in the provinces, but in London (and for seven miles around) they were still subject to the jurisdiction of the College of Physicians.

In many cases they were granted the status of *licentiate* in the college, in contrast with the *fellows,* who alone exerted control and who ordinarily were exclusively Oxford or Cambridge graduates. The struggles between the fellows and the licentiates we shall discuss again in another chapter. It suffices for the moment to stress the new opportunities open to apothecaries. The ambitious among them could indeed acquire an excellent medical education, even though their Oxford-trained competitors looked down upon them. Such an attitude was bound to persist until the Scottish universities tightened their standards and gave up granting degrees *in absentia.* The significant features, however, were the breakdown of the monopoly which Oxford and Cambridge originally held, the growth of genuine competition, and the greater freedom and opportunity which the apothecary enjoyed as a result.

The civilization of the mid-eighteenth century differed profoundly from that of the late seventeenth, and this difference in turn enabled competition to flourish, at first timidly, but eventually triumphantly. The reactionary attempts of the old guard to block all progress we will mention again.

Oliver Goldsmith, writing in 1759, made certain comparisons between Edinburgh and Oxford that well exemplify the old and the new:

> Skill in the professions is acquired more by practice than by study, two or three years may be sufficient for learning their rudiments. . . .
>
> The man who has studied a profession for three years, and

> practiced it for nine more, will certainly know more of his business than he who has only studied it for twelve. . . .
>
> Four years spent in the arts (as they are called in colleges), is perhaps laying too laborious a foundation. Entering a profession without previous acquisitions of this kind, is building too bold a superstructure. . . .
>
> Edinburgh only disposes the student to receive learning; Oxford often makes him actually learned.
>
> In a word, were I poor, I should send my son to Leyden or Edinburgh. . . . Were I rich, I would send him to one of our own universities. By an education received in the first, he has the best likelihood of living; by that received in the latter, he has the best chance of becoming great.[31]

The tide of medical education definitely turned in the eighteenth century, not without turbulence. Public need eventually destroyed the rigid monopoly originally established by Henry VIII. The first half of the eighteenth century witnessed decisive initial steps whereby, after further struggle, adequate education and adequate opportunity became available to all those who wished to practice medicine.

II

Quack and Empiric

In early eighteenth-century England a great many people gave medical advice and prescribed drugs. Such practitioners fell roughly into three categories. There were the physicians who had acquired a long formal medical education; there were apothecaries, less well educated, who had learned by apprenticeship and practical experience; and there were laymen who had done neither. For example, innumerable ladies of the manor, squires' or ministers' wives, went around trying to help the sick. Some were well motivated, with fine humanitarian impulses, others were officious busybodies who enjoyed imposing their will and their remedies on hapless parishioners. These well-meaning people knew little, and their practice was the crudest, but at least there was no financial gain involved. On the other hand there were many—

women and men alike—who made a good living from this irregular medical practice.

The regular physician, whose hard-won medical degree represented many years of intense study, looked down upon the other groups; but only when financial matters intervened did this disdain change to intense opposition, as in the conflict between physicians and apothecaries described in the previous chapter. Among the epithets freely bandied about, were "quack" and "empirick," words of considerable reproach often used quite interchangeably. The quack, as we have noted, was sometimes equated with the apothecary—one who gets no professional fee for advice but makes the patient pay disproportionately for cheap medicines. This is a rather narrow definition, stressing the economic aspect and illustrating the special enmity the physicians bore the apothecaries. The concepts involved were actually much broader, and the idea of the empiric deserves wider examination, apart from the question of fees and rewards.

In the eighteenth century "empiric" had various connotations. On the one hand it was considered synonymous with "quack" and "mountebank," but in a different sense it was opposed to "rationalist." Now these terms, "empiric" and "rationalist," are philosophical, referring to a theory of knowledge. How do we attain truth? The empiric, (in more modern usage, the empiricist) relies on "experience," while the rationalist depends on "reason." Without delving into philosophic subtlety, we can illustrate the significant difference through medical history. In the third century B.C., there were two distinct medical sects, each claiming to follow the true Hippocratic tradition. Hippocrates —let us assume for simplicity that we refer to a single person—was a great physician who observed carefully and accurately. He also held certain theories about disease. That is to say, he not only made precise observations, but on their basis he drew specific conclusions, reasoning with great acumen and judgment. Later physicians, however, split up into two groups, each claiming to express the true Hippocratic spirit but in effect each merely emphasizing a different aspect of the Hippocratic tradition. One group, the empirics, stressed the facts of observation, considering these to be of primary importance. The other emphasized theoretical inferences, or "reasoning" about the facts. This latter group, the dogmatists or rationalists, concerned themselves especially with the *causes* of disease.

Now, a cause is not directly given in experience but represents an *inference* from the data that have been observed. Such inferences, whose relation to observations may be close or remote, were the product of something called "reason." The rationalists attended to the question *why*. *Why* do things happen? What *explanations* can we give? The more the physician concerns himself with the problem *why,* the more he gets involved in theory. If his theory is sound and his reasoning is correct, the more complete will be his control over disease. The rationalist—in the eighteenth century, the educated physician—did things because there was a good, sound reason for so doing.

The contrary school, the empiricists, felt very little concern with the question, Why do things happen? They remained content knowing *what* was so without worrying about explanations. *What* foods are digestible? *What* medicines reduce a fever? *What* will cure a cough? These questions seemed more important than a theory of digestion, the causes of fever, or the mechanism of cough. Much better to stick to simple direct observation, to "facts."

The empiric acquired knowledge from chance observations or deliberate experience. He did not deny reason or inference, but he argued principally by analogy: what seems to work in one case will probably work in another case which is similar to the first. This type of reasoning remained closely geared to direct, relatively simple observations and did not concern itself with abstruse theory or speculative analysis. The empire did things because they worked, even if the reasons *why* remained unknown.

The apothecaries were certainly empirics in the philosophic sense, in that they had little concern with theory. They may or may not have been quacks. There were other individuals, however, eminent men of the highest stature, neither physicians nor apothecaries, who were in no sense quacks but who may be called medical empirics in the best sense.

One of these was John Wesley. The founder of Methodism wrote a medical text which enjoyed a fabulous popularity. The book, *Primitive Physick,* appeared in 1747, when the author was forty-four years old. During his lifetime—he died in 1791—there were twenty-three editions and many more after his death. The alleged thirty-second London edition was published in 1828. Its reputation by this time was too valuable to let pass and "new" editions appeared even later. There were at least seven American editions between 1764 and 1839, not to mention numerous translations into foreign languages.

Even the most critical would admit that the book was a success. As for Wesley's qualifications? Well, he had strong humane impulses; he was constantly in close contact with the sick and the poor, whose misery was for him a never-ending spur; and he was a well-educated man. During his long university background he undoubtedly read the great medical writers of antiquity. We know from internal evidence that he was acquainted with many seventeenth- and eighteenth-century medical authors. Any interested and well-educated man could without trouble read the works of Sydenham, Cheyne, Stahl, Hoffmann, Boerhaave, or Haller. And then Wesley made practical application of his reading, his observations, and his previous experiences with the sick.

Wesley had very little use for contemporary orthodox medicine. He set up an empirical system that, if we judge by popularity alone, worked at least as well as its more orthodox rival. Wesley expressed his doctrinary tenets in his Preface. The following account comes from the second edition, undated but thought to be no later than 1750.[1] The Preface, from which the first several quotations are taken, bears the date 1747 and is apparently reprinted from the first edition.

Only after the Fall of Adam, claimed Wesley, did sickness, death, and various hostile factors assail mankind, and medicine, or "Physick," come into being. Cure and prevention at first lay largely in exercise and temperance; more specific remedies were learned by experience, became traditional, and were handed down from father to child. Experience was the great teacher, and the wise man learned from experience and analogical reasoning: "It was easy to infer, if this will heal that creature, whose Flesh is nearly the same Texture with mine, then in a parallel case, it will heal me. The Trial was made. The Cure was wrought. And Experience and Physick grew up together." The original use of many medicines was learned by accident and by "experiment." If you are sick, you try an herb, and the sickness may end. "Thus ancient men, having a little Experience, join'd with Common Sense, and Common Humanity, cured both themselves and their Neighbors, of most of the Distempers."

At this point Wesley, for all his education, showed his basic anti-intellectualism. Men of a "philosophical" turn, he complained, were not satisfied. They wanted not merely to note phenomena but to account for them. "They examined the Human Body and all its Parts; the nature of the Flesh, Veins, Arteries, Nerves; the Structure of the Brain, Heart, Lungs, Stomach, Bowels; . . . the several kinds of Animal and Mineral, as well as Vegetable Substances. And hence the whole Order of Physick which had obtained to that time, came gradually to be inverted. Men of Learning began to set Experience aside: to form Theories of Diseases and their Cure, and to substitute this in Place of Experiments." This, of course, superbly expresses the empirical viewpoint. There is a basic mistrust of theory, a clinging to the immediate, simple, and direct. This represents, presumably, a temperamental bias which no quantity of argument or rational demonstration could change. Some personalities enjoy theory, even if speculative. Others prefer to remain close to immediate sense-perception. Some enjoy intellectual subleties and the juggling of abstract concepts; others do not. Wesley did not. He was a practical individual who, in medicine, wished to remain on a very simple level.

He defended his viewpoint, using as the best defense a vigorous attack. Theories, he pointed out, continued to increase, and old simple remedies, disregarded and forgotten, gave way to newer, more complicated ones. Rules and medical books multiplied enormously " 'till at length Physick became an abstruse Science, quite out of the Reach of Ordinary men." Physicians came to be regarded as something more than human and thus had a vested interest in keeping at a distance the generality of mankind who might pry into professional mysteries. Highly technical terms came into being, quite unintelligible to the common man. Physicians indicated that knowledge of anatomy and natural philosophy was necessary to medicine and "those who understood only How to restore the Sick to Health, they branded with the ignominous name of Empiricks." Medical remedies became more and more complicated and esoteric.

Wesley believed in the common man and saw no reason why medicine, with its humble origins, should exalt itself so proudly. He was one of those who tried "to reduce Physick to its Antient Standards: who have labored to explode out of it all Hypotheses, and fine-spun Theories, and to make it a plain, intelligible Thing as it was in the Beginning: having no more mystery than this: 'such a medicine removes such a pain.'" In line with these concepts Wesley maintained that neither natural philosophy nor even anatomy was necessary to cure human disease, nor "any Chymical, or Exotick or Compound Medicine." The medicinal virtues of single plants ordinarily sufficed. Except in rare instances, every man of common sense could prescribe for himself or his neighbor.

Wesley anticipated Hahnemann in demanding single remedies rather than compounds. He declared that, according to experience, one drug will be as effective as twenty put together. Why, then, add the other nineteen? Perhaps only for the profit accruing to apothecary and the doctor? A cynical view, indeed, and probably not unfounded in fact.

Wesley wrote his little book, *Primitive Physick,* to bring the healing art within the reach of all. The price was only one shilling. For this sum the purchaser received a series of remedies, arranged under various diagnostic heads. The patient had to diagnose his own disease, ordinarily quite easy except when there was a "complication of Disorders," in which case he should consult a physician. Under each disease there was one or more remedies offered. If the first did not work, the patient was urged to try the second, and if that failed, to continue right down the list. Wesley maintained that what cured one person might not cure another and that even what favorably affected a given person once might not cure him another time. Hence, thought Wesley, a variety of prescriptions was necessary. And in addition to specific remedies he advised temperance, moderation, rest, exercise, and faith in God.

The actual remedies need not detain us long. Wesley stressed the use of water, externally and internally. Cold baths and abundant water to drink were favorite prescriptions. For a "cold," as an example, he recommended, "Drink a Pint of *cold water* lying down in Bed: or, a Spoonful of *treacle* in half a Pint of Water,"[2] a remedy hard to improve upon even after two hundred years of scientific progress. For the measles he was equally sensible, advising "drink only thin *water-gruel,* or *milk and water,* the more the better; or, *toast and water.*"[3] On the other hand, for a cancer, if a cold bath did not cure, he prescribed "a Mellow apple, cut off the Top, take out the Core, fill the Hole with hogs-grease; then cover it with the Top and roast the Apple thoroughly. Take off the Paring, beat the Pap well, spread it thick on Linnen, and lay it warm on the Sore, putting a Bladder over it. Change this every twelve or twenty four Hours."[4]

This actually was not such bad advice, if we remember that "cancer" in the eighteenth century was not nearly so sharply defined as at present. Cancer was ordinarily a form of "scirrhus," that is, a hard swelling which might be neoplastic but more often was a non-specific inflammation. An inflammation might very well benefit from a hot poultice, often repeated. If the swelling improved under Wesley's treatment, we today would call it inflammation, but Wesley might readily think of it as cancer. There was no easy way to establish the diagnosis or to tell, in a recovered patient, just what disease he recovered from. This difficulty, unfortunately, is just as rife in the twentieth century as in the eighteenth.

It is not our purpose to discuss the many various remedies, or to analyze their merits or demerits. A quite satisfactory discussion along these lines can be found in Dock's study[5] and, to a lesser degree, in Riddell's.[6]

Wesley stands out as the best type of empiric. Scrupulously honest and sincere, he was an anti-intellectual who rested content with direct experience. His notions of "experiment" bear no relation to modern scientific concepts of knowledge. All he wanted was to "try it and see." Having seen, he was willing to stop. Once convinced of an empirical connection between a remedy and a disease, he felt no further urge to analyze. He did use a very elementary "conceptual" framework—since absolutely pure empiricism is totally dumb and inchoate, unable even to use words—but he restricted his "rationalism" to the barest minimum. He thereby avoided error, he thought, although he also lost the chance of achieving any deeper mastery over nature.

Rather different is the case of the English philosopher George Berkeley, who also dabbled in medicine. The brilliant achievements for which he is most widely known he accomplished at an early age. By the time he was twenty-five (in 1710) he had already formulated and published his major contributions to philosophy. His interests ranged widely. In middle life he spent three years in the Rhode Island Colony, where, among his other studies, he became familiar with certain medical practices. In 1734, several years after his return to Great Britain, he was appointed bishop of Cloyne. Cloyne is a small town about fifteen miles east of Cork, in the southernmost part of Ireland. Berkeley wrote, a little plaintively, that he was living in a remote corner among poor neighbors. For want of a regular physician these neighbors often consulted him for their medical needs.[7] We can assume that Berkeley encouraged such consultations, for here was a fine opportunity to use his medical knowledge and to test out his remedies. He had no compunction in practicing medicine, nor was he at all afraid of what physicians might say. In one place he said: "Truth is truth, and from whatever hand it comes should be candidly received. If physicians think they have a right to treat of religious matters, I think I have an equal right to treat of medicine."[8]

While living in Rhode Island, Berkeley became acquainted with tar-water, a medicine produced by shaking one quart of water with one quart of "tar" (that is, the resin obtained from evergreens) and allowing the mixture to settle. When a glass was poured off, it was replaced with fresh water and the whole shaken again. This substance was used in Rhode Island as a preventive against smallpox. Berkeley in Ireland modified the preparation. He used a gallon of cold water to a quart of tar, mixed thoroughly, and allowed to stand covered for three days. Then he poured off the clear supernatant fluid to get the medicinal agent, tar-water.

Berkeley wrote extensively on the virtues of this medicine. Because he indulged widely in theoretical discussions and argument, we might hesitate to call him an empiric like Wesley, and, since in *Siris* he also became abstrusely metaphysical, we may doubly hesitate. But, if we examine his theories more closely, we find a basic underlying empiricism: Tar-water cured diseases. This is the brute inescapable fact of which he was convinced, and in the demonstration of which he presented extensive evidence. Others might not accept the evidence, but, rather than enter into controversy, he said that he would "leave the decision to time and trial."[9] Again, he stated clearly that he put his views to the test, "and on those trials is founded my opinion of the salutary virtues of tar-water, which virtues are recommended from, and depend on, experiments and matters of fact, and neither stand nor fall with any theories or speculative principles whatever."[10] This is the very essence of empiricism: we start with experience which cannot be controverted, that is, with *facts* which are real and inescapable, and do not depend for their force on any theoretical consideration. It is not necessary to have general premises, deductions, or inferences.

But Berkeley went on to say that his high opinion of tar-water was not "blind empiric rashness, but rather the result of free thought and inquiry, and grounded on my best reason, judgement, and experience." Obviously there is a distinction between "blind empiric rashness," of the type we will discuss later and the enlightened empiricism which Berkeley, one of the world's greatest philosophers, ardently supported. Berkeley recognized what all too many have ignored, that "experience" is not a simple thing, nor is a "fact" always self-evident. Experience, and the facts it contains, requires evaluation at all times. "Reason, judgement, and experience" are the tools with which to evaluate experience. By their use we progress from an uncritical acceptance, or "blind empiric rashness," to sound empirical beliefs. Now exactly the same tools, "reason, judgement, and experience," also serve the rationalist. Their use distinguishes wild and unsupported speculative hypotheses from sound and fruitful theory. The basic tools are the same for both empiricist and rationalist. The difference lies not in the tools but in the material to which the tools are applied. Berkeley, as an empiricist, concerned himself primarily with questions, What are the facts? He marshaled a considerable amount of evidence and drew on extensive experience to establish the facts. But he was not content to remain on the simple experiential level. He also indulged in various theoretical analyses, which Wesley, for example, entirely avoided. As we shall see, Berkeley transcended even medical theory to indulge in abstruse metaphysics, but in these flights it is the philosopher who speaks, not the medical empiric. There is no doubt that from the viewpoint of logic Berkeley's medical approaches had a dual root, of which sound empiricism was primary; but the other root, based on theory, cannot be entirely ignored. Berkeley illustrates the difficulties attendant upon any artificial separation between empiricism and rationalism. Yet, historically considered, he is classified with the empirics.

Berkeley based his medical teachings about tar-water on many years' experience. To him it was an observable fact that tar-water cured diseases, especially fever. For example, he declared that those with very bad fever could be cured by drinking, in bed, a large glass of tar-water every hour and that those cured in this fashion recovered their health at once, while those who had been cured by the usual methods of the day—varied and copious evacuations, especially bleedings and purgings—"often languished long, even after the fever had left them, before they could recover of their medicines and regain their strength."[11] Unquestionably this was a very accurate observation.

He drew upon affidavits for forceful supportive evidence. In a ship off the coast of "Guinea," the captain attested that, of 170 Negroes attacked by smallpox, the only one who died was one who refused to drink tar-water. The other 169 all recovered by drinking tar-water without any other medicine, and this in spite of the heat and, to use an understatement, the "incommodities of the vessel."[12] This, of course, could it be accepted at face value, was a beautifully controlled epidemiological experiment. To some it was convincing—certainly John Wesley highly recommended tar-water as a remedy—but all did not agree. In the 1740's there grew up quite a polemic literature, the "tar-water controversy," regarding the merits of the fluid.

Berkeley thoroughly understood the experimental method. If we have an empirical assertion that such-and-such is a fact which some people affirm and others deny, how can we establish (or disprove) the validity of the assertion? Berkeley, as we have said already, was willing to abide by the verdict of time and trial, but he did not mean mere random trial. He firmly grasped the notion of controlled experiment. He suggested that patients should be "put into two hospitals at the same time of the year, and provided with the same necessaries of diet and lodging: and, for farther care, let the one have a tub of tar-water and an old woman; the other hospital, what attendance and drugs you please."[13] This experiment, had it ever been performed, would have determined whether Berkeley's claims were valid and would have furnished a model for future scientific experiments. But the trial was never carried out.

Berkeley, obviously, was interested in a sound empirical approach. But unlike Wesley he had a sound respect for theory and wanted to explain his observations, even though such explanations were secondary, in validity, to the observations themselves. He was widely read in contemporary scientific and medical doctrine and utilized much Boerhaavian anatomy and physiology, together with some chemical principles of the times. Berkeley's terminology was by no means lucid, especially when, in *Siris,* he delved into rather fanciful explanations. Within the resinous substances of the plant, he thought, existed an "acid spirit" and salts which the action of cold water dissolved out. The volatile oily salt was the acid spirit which contained the medicinal virtues. Disease, he thought, was largely a matter of obstruction to blood-flow. Tar-water cleansed the finest capillaries much better than water alone, which distended the vessels and weakened their tone; it worked better than mercury, which, although a powerful cleanser by virtue of its mass, momentum, and velocity, broke down obstructing matter but also broke down the fine fibers. Tar-water, on the other hand, softened and gently stimulated the capillaries.[14]

Leaving chemical speculations behind, Berkeley soared to imaginative and mystical heights with his concepts of invisible Fire, pure "Aether," Spirit, and Light. With the fanciful aspects and metaphysical speculations, we have at present no concern. *Siris* is a book not of medicine but of philosophy; it started with the facts of tar-water and then, leaving empiricism behind, rose by a "chain" of reasoning to the metaphysical. In this work it is mostly Berkeley the metaphysician who speaks. However, in the various letters to Thomas Prior and Dr. Hales it is the empiricist who writes. Berkeley, as the advocate of tar-water, exemplified the best type of eighteenth-century empiric.

At the opposite end of the range of connotations implicit in the term "empiric" was the mountebank, who still survives as the patent-medicine vendor of present-day carnivals. During much of the eighteenth century he was a significant figure who literally mounted a bench or platform from which he harangued the public. Even then he had a low reputation but flourished nevertheless. The technique of the mountebank has not changed very much. Ben Jonson's villain, Volpone, provides a brilliant picture of the early seventeenth-century version when, to further an intrigue, he assumes the character of a mountebank and gives a typical exhortation that is still high comedy. The *Volpone* includes also a song that epitomizes the whole process:

> Would you be ever fair and young?
> Stout of teeth and strong of tongue?
> Tart of palate? quick of ear?
> Sharp of sight? of nostril clear?
> Moist of hand? and light of foot?

Or, I will come nearer to 't,
Would you live free from all diseases?
Do the act your mistress pleases,
Yet fright all aches from your bones?
Here's a med'cine for the nones.[15]

About a century and a half later we find the same theme, phrased somewhat differently but expressing the same message. In the remarkable volume *The Life and Extraordinary History of the Chevalier John Taylor,* we read a mountebank's speech that, if never actually delivered verbatim, certainly reflects the spirit and style of that profession. A few quotations are of interest:

> The nature of Good, my worthy Countrymen, is to communicate itself. . . . Good is no Good, except it is diffused. Good, like a Dunghill, is good for nothing, till it is spread about. . . . No more is a Heap of Gold itself. . . .
>
> The Mountebank is like a Tinker in his Trudge, the Judge upon his circuit, the Bishop in his Visitation, the doctor in his Country Call, and WHITEFIELD in his province.
>
> The Mountebank, or Travelling-Doctor is like the Sun, the Patron of his Art, he shines out far and near; he blazes as he travels. . . .
>
> My Medicine, like the Manna in the Wilderness, shall fall in showers around you, and restore your Peace. . . . My Manna I spread about, my Gold I circulate; my Virtue shall revive you, my knowledge shall preserve you.
>
> I am your Champion in the Cause of Health. I trample down the Dragon called Disease. . . . Behold the Bullet that shall demolish them [referring to hospitals] that rob the Purses of the Public to bolster up Lazy and the Ignorant. This Pill, this mighty Pill, when shot from the artillery of my knowledge, shall lay them all in Rubbish. . . . This Pill shall counteract PANDORA's Box, and shall drive away all physical and moral Evils. . . .
>
> Let no Horrors henceforth hang upon the fierce Embrace of rapid Lovers, or damp their extatic Joy with Apprehensions

> of the foul disease. This NOSTRUM is your Sword and Buckler; this shall beat down every FRENCH Antagonist. . . . Inestimable Pill! . . . It is a Miracle of Military Virtue, at perpetual War with every Thing that gives Disease.[16]

The mountebank, always itinerant, thrived especially in the country districts, where there was little or no control over the practice of medicine. In Edinburgh, however, as late as 1672, itinerant physicians received permission from the authorities to erect a public stage for the practice of their profession but were forbidden to have any "rope dancing," that is, presumably, any sort of side show.[17] And in 1684 a German mountebank received a license to erect a stage in Edinburgh, even though the College of Physicians, then in existence, actively opposed the petition.

The mountebank adopted a particular type of advertising, namely, a public harangue from platforms or stage, during which he would praise his own abilities and might even perform various cures in public as further advertising. The mountebank, however, was merely one type of quack. With spreading sophistication the public bench or stage, erected for the purpose, disappeared into the rustic hinterlands; yet the quacks flourished vigorously in metropolitan areas. Richard Steele, genial Spectator, quite despaired when he saw people deceived by "successive Cheats and Bubbles." He thought it strange that anyone is "so ignorant, as not to know that the ordinary quack doctors, who publish their Great Abilities in little brown Billets, distributed to all who pass by, are Imposters and Murderers."[18]

If we try to pin down the meaning of the terms, we find Johnson's dictionary defining the quack, in general, as a boastful pretender to arts he does not understand. More particularly, in medicine the quack is a vain boastful pretender to medical knowledge, who proclaims his own medical abilities in public places. This definition has three significant features. That a man is vain and boastful does not by itself mark off a quack from innumerable more orthodox and unstigmatized physicians. Modesty is a virtue, its lack is a social defect, but a man can be quite reputable despite vanity and boastfulness.

The two other items are more significant. The quack was a pretender, basically ignorant, pretending to know when he did not. Also, he advertised. A variety of techniques was available, including handbills, posters, newspaper advertisements, and the carnival tactics of the mountebank. The quack developed advertising techniques to a point that even the modern huckster need not disdain. The meaner sort of itinerant quack rode horseback and dispensed his pills and plasters on a small scale. But the more pretentious drove in a coach and four, with numerous servants who had a dual role. Besides attending their master, they doubled as advertising agents to "puff" the great doctor. The quack entertained lavishly, giving dinners, alms, free drinks, while the servants circulated and insinuated all sorts of laudatory rumors. Then the quack would make public performances or private consultations, cashing in on the publicity he had obtained.

There is a fallacy in this definition of the quack, since there need be no necessary relation between one who is ignorant and one who advertises. We merely assume it on the theory that, if he really knew anything, he would not have to advertise but could get along quite well through more orthodox means. This assumption is not valid, since there were some quacks, especially in ophthalmology, as highly skilled in their fields as the most reputable physicians. Their origin may have been humble, their education irregular, their customs and practices unorthodox, and yet, by the standards of their times, they were still very skilful.

William Read, who died in 1715, was such a one. Originally an illiterate tailor who became an itinerant quack and on occasion a mountebank, he devoted himself especially to diseases of the eye. He achieved quite a reputation. How much was deserved is rather hard to tell now, but a sample of his advertising handbills is well worth quoting:

> Oculist and chirurgical operator, who after 22 years travel and practice, hath Acquired the true Method of performing all Curable Distempers, Incident to ye Eyes, as Couching of Cataracts, Glaucomas, Suffusions . . . he does these things upon all Persons before he takes any Money, which is a Confirmation that he is not one of those many Circumforaneous Pretenders that Infest this Land. . . . You may have faithfull Advice of him whither [*sic*] you are curable or not. And may be Spoken with at his Lodging from 8 in the morning till 6 in the Evening.
>
> Hee cureth ye Poor of Blindness, Cancers, Wenes, Heair lipes, Wry Neckes and Dephness for Charity.[19]

This was indeed a fine recommendation, albeit self-composed. Undoubtedly Read did a great deal of charity work. There is an advertisement on record, after the Battle of Malplaquet, that he would treat all wounded soldiers gratis, if they brought appropriate certificates from their officers.[20]

By the standards of the times he was probably a competent operator, regardless of his meager educational background. At any rate, he achieved two high honors: he was knighted in 1705 and was also appointed oculist to Her Majesty, Queen Anne. He became quite wealthy, mixed in the very best society, and altogether achieved an enviable place in the community. Detractors and lampooners he had in abundance, but on the whole Read compares very well with his less flamboyant and better-educated colleagues.

In the same category as Read, but living entirely in the eighteenth century, was the notorious John Taylor, called "the Chevalier." Born in Norwich in 1703, he wrote modestly of this city, "It is possible that its having been the place of my birth, may not be one day judged unworthy the notice of posterity."[21] His autobiography cannot be accepted as reliably factual, but various accessory sources establish the main outlines of his life. He was "bred to physic," serving as an apothecary's assistant in London in 1722 and studying surgery at St. Thomas' Hospital under William Cheselden. At first he practiced as surgeon and oculist in Norwich, but for reasons not quite clear decided to devote himself entirely to eye diseases. Once this decision was made, he frankly disclosed why he had to travel. The great problem was how to get enough patients to practice on so that he might continually improve. If he stayed in one place, any "miscarriage," that is, any unsuccessful treatments, would ruin his practice. The only way to acquire experience and skill was to travel, to become an itinerant oculist. This, he knew, would expose him to the censure of his colleagues, for reputable men with regular medical training did not travel in this fashion. Those who did travel, pretending to "particular secrets" in some branch of medicine, "were generally wretches of little honour and less ability."[22]

Although he would never stay very long in one place, in his travels he was "secretly informed" by correspondents of the results previously wrought. He knew that sometimes the results would be bad, but this was only to be expected. At any rate, travel he did, first over most of Great Britain and then abroad. In 1733 he received an M.D. degree from Basel and, in 1734, from Liége and Cologne. Returning to England in 1735, he was the next year appointed oculist to George II. But he continued his travels from one country to another, returning to England at intervals. During these years he wrote extensively on ophthalmological subjects, writings that, with all their faults, are by no means to be despised. He died abroad in 1772, having left England for good in 1767. His autobiography of 1761 gives a record of his extensive travels.

That he had very considerable operating skill seems generally agreed, but that he advertised himself like any quack seems also established. Certainly his autobiography is a remarkable farrago of self-laudations, consisting largely of anecdotes about himself, together with all the self-adulation, with or without evidence, that he could possibly crowd in.

Correlative to the *History,* written by himself, is the *Life,*[23] supposedly written by his son. This, now considered to be the work of one Henry Jones (1721–70), is almost the direct opposite of the autobiography. This *Life,* although it does show some respect for his technical skill, seizes every possible chance to denigrate his character. He appears a vile lecher, an unprincipled blackguard, a cheat. The book reads like a picaresque novel, and its relationship to fact is probably tenuous. Nevertheless, it gives a picture of an itinerant quack, albeit a skilful one, that probably has a modicum of truth as applied to John Taylor and a great deal of truth concerning eighteenth-century quacks in general and their servants who connived with them.

Both Read and Taylor were empirics in that they acquired all their learning through experience. They were quacks in that they advertised themselves prodigiously and boastfully. Apart from their technical skill they were very ignorant when compared to the regular physicians. Taylor, to be sure, had the advantage of some medical education, even if not of the best quality, and he did possess Continental M.D. degrees. Nevertheless, he was a quack to his contemporaries. Hogarth's famous engraving, "The Consultation," groups Taylor with the "bone-setter" Mrs. Mapp and the famous "Spot" Ward.

Joshua Ward we must rate distinctly lower than the itinerant oculists. He was not "bred to physic" but started life as a drysalter, that is, a dealer in crude chemicals and salted food products. This background introduced him to the world of chemical substances. Little is known definitely about his early life. In 1715, after the unsuccessful demonstration for the Pretender, Ward fled to France. Whether his departure had a political stimulus, or only an economic basis, is not clear. In France he supported himself by selling nostrums which made him famous and wealthy. In 1733 George II pardoned him and he returned to England to continue a very successful career. He cured the King's dislocated thumb, for which the gratified monarch showered favors upon him. Besides the King, Ward had many famous patients, including Henry Fielding, Edward Gibbon, and Lord Chesterfield. Fielding, indeed, whom he treated unsuccessfully, spoke very highly of Ward and made excuses why the cure was not successful. Fielding realized that there cannot be an infallible nostrum to cure every patient and thought his own case must have had something special which resisted the medicines that had cured thousands. At the same time, the great novelist declared, "Obligations to Mr. Ward, I shall always confess; for I am convinced that he omitted no care in endeavoring to serve me, without any expectations or desires of fee or reward."[24]

Ward had, for his chief stock in trade, two remedies, a pill and a drop. Both of these, it was later shown, were strong preparations of antimony, but they became very famous. The pill even involved him in political difficulty. Ward was very kind to the poor, perhaps because much of his charity was charged up to the King's expense. Because he gave away much medicine, he was accused of engaging in papist plots, by the following oblique reasoning: Ward was a papist. He distributed his pills gratis to the poor, through a beneficent woman who was also a papist. Therefore, it was alleged, the pills must be a deep-laid plot to introduce popery. Fear of indoctrination-through-medicine is a rather advanced and malignant phase of witch-hunting.

Ward had many bitter opponents, who goaded him into the foolish move of going to law with one of his assailants. During the trial his ignorance and his hollow pretensions were exposed. It came out that he had no medical knowledge, only a slight knowledge of pharmacy, and that his pill and drop were not suitable for general use. Nevertheless, disclosures did not lessen the public faith in Ward; he flourished more than ever and, when he died in 1761, he left a large fortune. He bequeathed his remedies in such a way that all the profits went to certain charities. For a while after his death the sales continued, yielding considerable income; but without the master's personality they soon lost favor and disappeared.

Ward personified the very quintessence of quackery. Without medical knowledge or training he exploited a medicinal substance, winning the public favor and a large fortune in the bargain, undoubtedly curing a great many sick people, if only by indirection. There can be no question that he killed a great many others, but this, to his contemporaries, was of lesser significance. He probably was not a hypocrite, a conscious liar, or a deliberate fraud. Probably he firmly believed the merits he so boldly asserted. To support his faith he could certainly point to good evidence: he could bring forward all the cases which the medical profession had abandoned as hopeless but which he had successfully cured.

This is evidence hard to discredit. Nothing succeeds like success. When orthodox practitioners gave up a case as hopeless, and when that same case recovered under Ward's ministrations, then the quack could smilingly disregard the sneers of regular physicians. Or at least he could so do if the recoveries were at all numerous. The public is impressed more by results than by excuses. The failures of the regular practitioners and the successes of the so-called quacks are quite impressive when properly advertised. Somehow the failures of the quacks do not attract an equal attention.

A still different type of quack was the notorious James Graham. A charlatan tinctured with mental instability, Graham had had some early medical training at the University of Edinburgh. After traveling in America as an itinerant oculist and aurist, he returned to England to promote the financial rewards supposedly inherent in electricity. His famous Temple of Health created quite a stir with its Celestial or Magnetico-Electrical Bed. Copulation in this bed was supposed to yield a very superior pleasure and ecstasy, and in childless couples would lead to immediate conception. Needless to say, rental of this bed was not cheap. Graham had an immense but short-lived vogue in London, but he was eventually forced to retire to the provinces, where he spent considerable time in jail and was eventually adjudged insane.

There is little to be gained in trying to enumerate all the famous quacks who abounded in the eighteenth century. The books of medical anecdotes of the last century—Jeaffreson[25] and Everitt[26]—contain abundant material for the curious and, while not always accurate, maintain the general pungent flavor.

The term "quack," or "empiric," included a wide variety of individuals differing in education, intellectual power, skill, character, and mental stability. These irregular practitioners of medicine, who did not possess the formal training of regular physicians, flourished because the regular practitioners could not satisfy community needs. Quacks had this charter for existence: they satisfied a certain number of people from whom they drew support. When they ceased to satisfy, their support stopped and they disappeared. The very existence of irregular practitioners indicts the orthodox physician for failing the community. It is not that the quacks adequately satisfied society's needs. It is only that, since the regular physicians failed, the quacks had their turn, and, in so far as the quacks were temporarily successful, they gained renown and wealth. But when they too failed, then they disappeared.

We can ask, as a legitimate question, Why did the physicians fail? The first obvious answer that comes to mind is that they did not know enough and did not know how to take care of people. Compared with the twentieth century, medical knowledge then was pathetically rudimentary. But ignorance alone, no matter how vast, is not a satisfactory answer, since ignorance is always a relative affair. Physicians failed because, for one or another reason, they did not give the patient what he wished. Obviously, sometimes patients desire the impossible, and anyone who promises it is either a charlatan or a fool. At other times the patient desires attention and help which are perfectly reasonable and possible, but which, somehow, the physician does not furnish. The various quacks and empirics we have discussed fall roughly into two groups: there were the fools and charlatans who promised the impossible, and there were the reasonably conscientious and honest who tried in their own way to supply the needs which the regular physicians had not provided. Intermediate forms we must omit.

The great majority of empirics—the outright frauds we may dismiss—tried to do right according to their own lights. Sometimes these lights were rather dim and, when such individuals began to make a lot of money, the orthodox physicians objected. The empirics, they claimed, used crude and uncouth methods and did not have the educational background needed to treat the ills of mankind. How much background does a man need to practice satisfactory medicine? This, alas, is a perennial problem, as acute in the twentieth as in the eighteenth century. No simple answer can be given, but we must always consider that bias and prejudice may perhaps play an unduly large role in determining the desired background.

Now all medical training, at all times, has included both theoretical and practical aspects, the rational and the empirical. This distinction has survived for two thousand years. At the beginning of the eighteenth century it gained special attention in the quarrels of physicians and apothecaries, and, when the century ended, it was still a matter for argument. James Gregory, whose acid pen we will meet again, emphasized how the terms had lost their original significance. His discussion has a completely modern ring. Originally, he pointed out, physicans were divided into

> Dogmatists and Empirics: the former professing to understand the nature and causes of disease, and the mode of operation of the remedies which they employ to cure them; the latter pretending to no such knowledge, professing to despise such speculations, and to trust to experience alone. . . . the names of the contending sects, which were originally honorable terms of distinction, have long since . . . become common terms of reproach and contempt. Dogmatist originally meant exactly what we should now call men of science: but has long been understood to mean an arrogant, positive, uncandid man, who despises experience, and teaches or speaks in a style of absolute authority, and is obstinately attached to opinions unsupported by experience, and often repugnant to it. Empiric means literally one who regards experience; but has long been under-

stood to mean an ignorant impudent pretender to physic: it is just synonymous with Quack or Mountebank.[27]

The original meanings had become corrupt, and this corruption emphasized the schism that had arisen among medical practitioners.

But by the end of the century there was also a movement to heal the schism, to reunite once more the divergent points of view. Thomas Percival wrote two brilliant essays on the subject.[28] In one he praised the empiric at the expense of the rationalist, while in the other he did exactly the reverse. Now obviously, an empiric cannot at the same time and under the same circumstances be both better and worse than the rationalist. Percival chose the best aspects of each and showed that the best practice of the empirics was vastly preferable to the worst of the dogmatists. But the converse also held true. The best of the dogmatists (or rationalists) was preferable to the excesses of the empirics. We cannot forsake experience, but neither can we disregard a knowledge of causes. "Mere experience," he said, "whether derived from books, or acquired by personal observations, is insufficient of itself to qualify us for judicious and successful practice." There must also be a knowledge of the "animal economy, of the influences or external causes—the changes induced by disease," and of consistent rational theory.

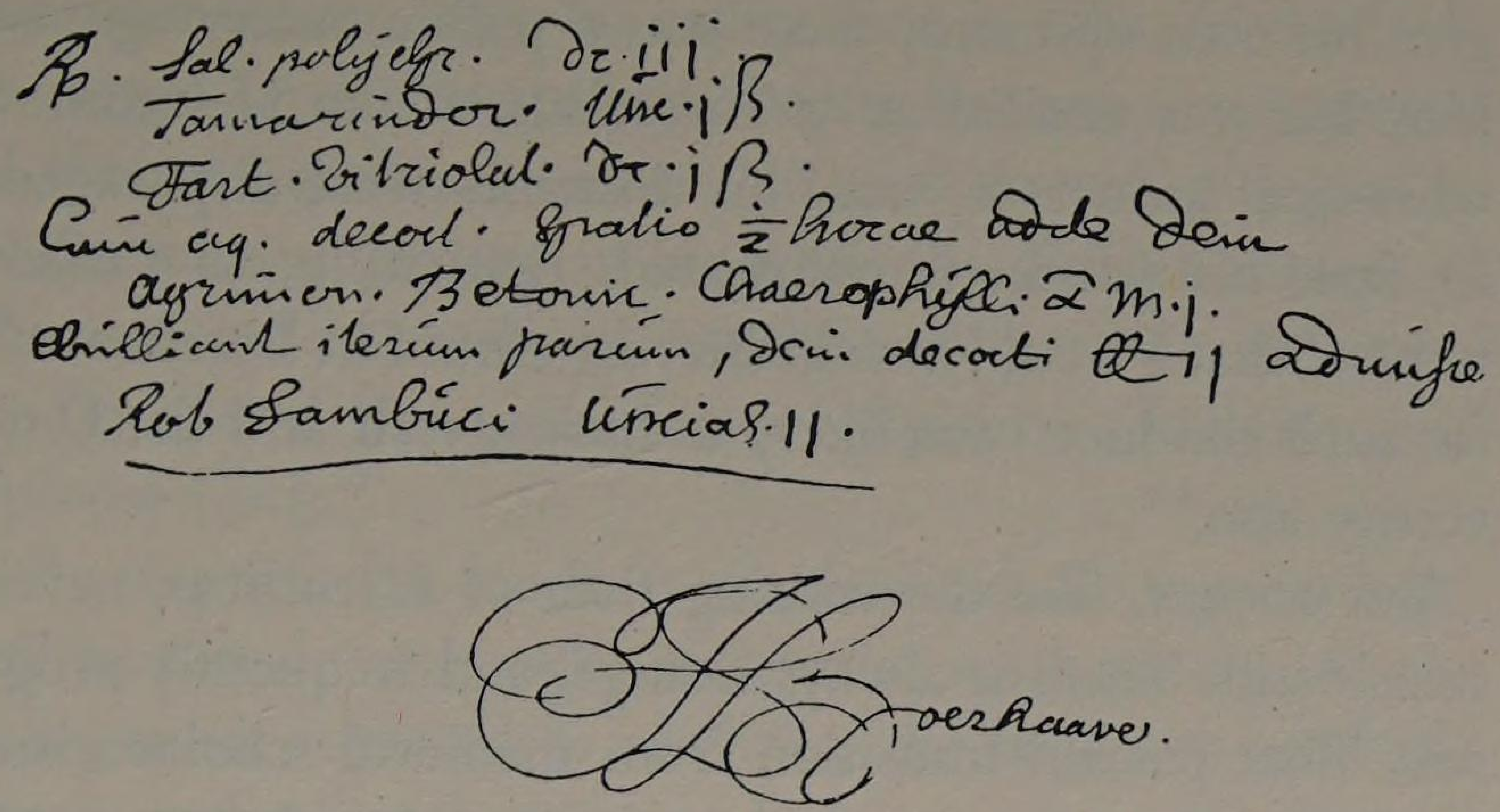
When we consider the ideal form of each, there is no basic difference between empiric and dogmatist. Both start with experience and both try to bring order into the fact of nature. "Experience" and "Reason" are only abstractions. Trying to keep them rigidly separate leads only to absurdity. Keeping them in due and proper proportion leads to truth and progress and to what we acclaim as the modern scientific method. By the end of the eighteenth century the excesses of each school were gradually becoming apparent, and there was a definite trend to achieve a true synthesis. New outlooks superseded the old and rendered futile the former controversies. In the nineteenth century this synthetic approach yielded tremendous practical advances, which constituted the ultimate touchstone. A new scientific method, combining the best of the old, proved itself practically superior and created new standards for sound medical education.

From the safe vantage of the twentieth century we can readily appreciate the eighteenth-century empirics, who scorned the early eighteenth-century dogmatists. The latter, freighted with ancient and medieval authors, considered such lore essential for "rational" practice of medicine. But the useless lumber was too onerous, the training period too prolonged; the orthodox physicians were consequently too few in number and too expensive in practice to satisfy the public needs. These needs did not go unsatisfied. Instead, the vacuum was filled by "empirics," who dispensed with "unnecessary" theory, who streamlined the preparatory period, and who achieved just about as satisfactory results. This, however, was not a durable solution, for the empirics went to excess, opening the gates to the ignorant, the uncritical, and the fraudulent. The public ultimately suffered. A synthesis took place. Reputable physicians, jealously and under pressure, altered their training methods, discarding from their education the superfluous, antique, and outworn theories, the uncritical acceptance of dogma or unsupported hypotheses. They adopted a more open approach to experience; indeed, they could not help themselves, for great empirical advances in various phases of science compelled a reorientation. The old conservative outlook, however, offered a necessary discipline which helped achieve a synthesis of old and new that could be truly adaptive.

History suggests the following sequence. When dogmatists or rationalists become ascendant, when theory and "required background" become top-heavy, then empirics, who avoid excess theory, begin to flourish as a counterbalancing force. When empirics become ascendant, without adequate theoretical foundation, they sow their own destruction through insufficient standards, inadequate discipline. Each group acts as a check on the other. Eventually the excesses of each are purged away, and a new synthesis combines the best features of each, discards the worst. Then in Hegelian fashion, a new cycle commences.

The eighteenth-century empirics played a very important role in medical history. As a foil to the orthodox physician, they emphasized the latter's defects and hence to some degree were instrumental in correcting them. As unorthodox practitioners, they were not bound by rigid curriculum. They did not feel the dead hand of the past and could try to exploit every new idea. Often crude and brash, they were nonetheless salutary in exposing what was useless or unnecessary. Perhaps they went to extremes but only because of extreme defects elsewhere. And by their very extremes they acted as a leaven to true progress.

III



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H. Boerhaave.

Hermann Boerhaave, Systematist

In the entire eighteenth century the most influential physician was probably Hermann Boerhaave. He received his medical degree in 1693 and speedily rose, first to local prominence and then to international fame. His medical teachings quite dominated the first half of the eighteenth century and with somewhat diminished luster still maintained authority and prestige for the second half. William Cullen, commenting on his own student days in Edinburgh, says, "I learned the system of Boerhaave; and except it may be the names of some ancient writers, of Sydenham, and a few other practical authors, I heard of no other names or writers on physic; and I was taught to think the system of Boerhaave to be very perfect, complete and sufficient."

After Cullen became professor of theory of medicine in Edinburgh in 1766, he appreciated the full power of the

Boerhaavian tradition; when in his own lectures, he ventured to differ from the great Dutch physician and to give his own opinions, there was a public outcry against him. He was assailed as "a Paracelsus, a Van Helmont, a whimsical innovator," and his doctrines were disparaged. In fact, his patron remonstrated, requesting, as Cullen wrote, "that I would avoid differing from Dr. Boerhaave," for such conduct "was likely to hurt myself and the University also."[1]

But science, like the rushing river of Heraclitus, never stands still. There is always change, and frequently progress. That painful transition from medieval scholasticism to the modern empirical attitude introduced masses of new data, a wealth of new concepts. Ordinarily new concepts do not replace the old suddenly or completely; rather there is a transitional period of fusion, adaptation, and compromise. Eventually the old may be completely discarded but only after a considerable interval.

By the late seventeenth century the Greek traditions, modified by ancient and medieval authorities, had many new medical discoveries to assimilate—the circulation of the blood, the world of animalculae and particles disclosed by the microscope, the newer anatomical knowledge gained from dissection and from vascular injections, the rudiments of modern chemistry, and the concepts of Newtonian physics. The old order was changing, gradually. While still vigorous and even dominant, it could not disregard the new. It was Boerhaave's great virtue to fuse the old and the new into a well-organized complete system, blending fact and theory to satisfy the contemporary needs.

Boerhaave's life and education cast much light on the medical life of the times. He was born in a small Dutch village very near Leiden, in 1668. His father was a minister, his stepmother the daughter of a minister, and young Hermann was destined for the ministry. Brought up in a scholarly atmosphere, he applied himself very diligently to his studies, not only as a child but, with special intensity, after his father died in 1682. At the University of Leiden he studied divinity but was also attracted to the mathematical sciences. In 1690 he took a degree in philosophy, discussing in his thesis the distinct natures of the soul and of the body, in which dissertation, it is said, he "confuted the sophistries of Epicurus, Hobbes, and Spinosa [*sic*]."[2]

By this time his funds were exhausted. To support himself he tutored university students in mathematics. At the same time, attracted to medicine, he bagan to read the great medical authorities and was so intrigued that he finally decided to take a medical degree before engaging in ministerial duties. He read Vesalius, Bartholin, and Fallopius, attended public dissections, and dissected the bodies of animals. With this background he systematically read the Greek and Latin authors, starting with Hippocrates and continuing his readings to include the moderns, especially Sydenham. Boerhaave also became skilled in chemistry and botany. Finally, in 1693, he took his medical degree, writing his dissertation on the usefulness of studying the excreta of sick patients.

At last he felt ready to undertake his calling for the ministry but found himself hindered by unjust accusations of "Spinozism," that is, of atheism. Once, pleading for fairness, he had antagonized a demagogue who was at the time calumniating Spinoza. Thus, by simple extension Boerhaave acquired a reputation as a Spinozist. This was an almost insuperable hindrance to entering the ministry in seventeenth-century Holland, much as an established reputation as a Communist might preclude government employment in the mid-twentieth-century United States.

The hostility forced Boerhaave to renounce the ministry and to devote himself to medicine. He practiced medicine, pursued chemistry experiments, and, in addition, continued his mathematical and his scriptural studies. Apparently an excellent teacher, he began to give public lectures in chemistry. In 1701 he received academic recognition when he was appointed lecturer on the institutes of medicine at the University of Leiden. In 1709 came the promotion to professor of botany and medicine. In 1714 he became rector of the university and, at the same time, physician at the St. Augustine's Hospital in Leiden, where he introduced the modern system of clinical instruction. In 1718 he was also appointed professor of chemistry. He became perhaps the most famous physician in the world. It is said that a Chinese mandarin addressed a letter from China "to the Illustrious Boerhaave, physician in Europe" and that it reached him in an era when postal service was rudimentary. Illness forced him to curtail his activities, and in 1726 he resigned his academic appointments. He continued practicing, however, until shortly before his death in 1738.

Boerhaave's career helps explain the course of medical progress. He was a great student all his life but in the medieval tradition rather than in that of modern science. Trained to a theological career and taking his first degree in philosophy (that is, in "speculative" rather than "natural" philosophy), his first orientation was toward metaphysics and philosophic subtlety. In theology, written authority, as embodied in the Scriptures and in the works of the church fathers, was very powerful. But authoritarian supremacy is not quite consonant with that free empirical inquiry and inquisitive attitude that Francis Bacon had advocated. To be sure, Boerhaave was quite in the forefront of the scientific advances of the day. In mathematics, chemistry, and botany he attained considerable eminence, and in the latter two subjects he held professorial chairs and wrote textbooks. Yet, although he did engage in actual experimentation, his approach was basically that of the compiler: to the wisdom of Hippocrates, Galen, Aretaeus, and Celsus he tried to attach the more recently discovered knowledge and to fuse the whole into a satisfactory system.

Perhaps because of his profound scholarship and his respect for the past, he did not properly evaluate the new, nor was he sufficiently critical of the old. He paid lip service to the methods of inductive science; he decried "hypotheses," he lauded the importance of experience and condemned speculative theory, but he lacked critical acumen. Acumen, of course, does not represent merely some abstract mental quality. It depends, at least partly, on sheer quantity of information. Early in the eighteenth century abundant new observations had accumulated, but there was not yet that large mass of new and compulsive data needed to promote a truly vigorous empiricism. The old was still preponderant. Doctrinal conservatism prevailed. New data were being fitted onto an old framework instead of inspiring a new framework. Boerhaave's great contribution was to construct a polished doctrinal edifice, almost monolithic, that captured men's attention, satisfied their curiosity—and restricted their imagination. Here and there some of his assertions were demonstrated to be false, but in general his teachings persisted without much change throughout most of the century, acquiring in academic circles a revered character and an almost dogmatic authority. Only when new data were so overwhelming in quantity and significance that old concepts no longer satisfied, only when new theories, techniques, and attitudes replaced the old, when orientation shifted from the remote past to the immediate present, and the genuine obsolescence of the ancients was accepted, only then was Boerhaave superseded. This process required most of the eighteenth century, and meanwhile his doctrines, formulated and published early in that century, served as the standard for medical education and practice.

Boerhaave's most influential writings were the *Institutiones medicae,* first published in 1708, and the pithy, compact *Aphorismi de cognoscendis et curandis morbis* of 1709, which went through numerous editions and translations.

Institutes of medicine was a subject with no exact modern equivalent. It corresponded to a combination of physiology and anatomy with a little chemistry thrown in, all furnishing the background for clinical studies. Boerhaave's lectures[3] on the subject achieved a tremendous vogue. The *Aphorisms* was published in many Latin and several English editions.[4] It is most useful, however, with the superb *Commentaries* by Van Swieten. Gerhard van Swieten (1700–72) was Boerhaave's favorite pupil and assistant, who fortunately was a master of contemporary shorthand. He recorded Boerhaave's own comments on patients and thoroughly absorbed his master's attitudes, concepts, and general approach. In 1754 he published a set of commentaries on Boerhaave's *Aphorisms,* which is most readily available in the English edition of 1775 in eighteen volumes.[5] This monumental work not only offers the best translation of the original Latin *Aphorismi* but furnishes extensive analyses, detailed commentaries, abundant quotations from medical literature ancient and modern, and many clinical examples. To some extent the commentaries probably reproduce Boerhaave's own words, recorded verbatim by Van Swieten, but much of it undoubtedly represents the master's thoughts interpreted by the pupil. Regardless of the authorship of any particular paragraph, the *Commentaries* exhibit the essence and spirit of Boerhaave's doctrines, relatively little changed from the original teachings.

It is rather difficult to project ourselves backward two and a half centuries into Boerhaave's world. Our present medical concepts are now so ingrained that they cannot readily be divested. Yet to appreciate Boerhaave we must remember the limitations of his knowledge. He knew nothing, for example, about cells in the modern sense. Nor did he have any notion of bacteria or microbiological agents. Chemistry as we know it today was then in the earliest stages of gestation. Microscopy was rudimentary indeed, even though sufficiently advanced to demand and receive attention. Nevertheless, physiological investigations were numerous, yielding data that were becoming integrated into contemporary doctrine. Perhaps the greatest scientific advances were in mathematics and physics. Descartes, Leibniz, and Newton had established the principles of classical mechanics—movement of material particles in space and time according to the laws of motion—doctrines whose pervasive influence we shall see throughout Boerhaave's writings.

Boerhaave, by his own lights, was quite well versed in nature's mysteries. He knew a great deal. His own concepts were as real to him, and just as significant, as our modern notions of enzyme systems, antibiotics, or electron microscopy are to us. He had, moreover, the inner compulsion to explain, to systematize, to tie up all the loose intellectual ends into a neat system. There was a residual medievalism, where, instead of a *Summa theologica,* we had a *Summa medica.*

How, then, did he regard medical phenomena? He started with a relatively small number of primary concepts which, it must be emphasized, were grounded in concrete empirical evidence. These primary concepts he elaborated according to the demands of logic and "just" reasoning. The living body had two primary components, the fluid and the solid. The fluids taken collectively formed the humors of many different types. The solids, apparently so diverse, were all composed ultimately of fibers. The problems of disease received final explanation from these elements.

The ancient doctrine of the humors had undergone progressive elaboration and changes. Boerhaave's own concepts rested on experimental observations. For example, there was the analysis of blood. To examine blood under the microscope the procedure at that time was to draw a minute quantity into a capillary tube, dilute it with a drop of water, and observe the tube under the primitive instruments then available. What we now call "hemolysis" was quite unknown. Leeuwenhoek, and Boerhaave after him, would observe the red globules. But these, simply on standing, broke up, and in their place smaller yellow globules appeared. It was generally taught that a single red globule disintegrated into six yellow ones. Further observation showed that—again, after standing—the yellow globules vanished, and only smaller "pellucid" spherules remained in the ratio of six spherules to one yellow globule. The available equipment did not reveal any still-smaller spherules, yet reasoning by analogy compelled Boerhaave to assume progressively smaller particles, much smaller than the pellucid ones which, we recall, were each one thirty-sixth the size of a red cell.

This "modern" knowledge, which the new technical advances had made possible, enabled Boerhaave to criticize the Galenic humoral concepts. The four humors of the Greeks are well known. Boerhaave pointed out that the "blood" of Galen, far from being homogeneous, was merely the aggregate of the red globules which imparted the red color.[6] Galen's "yellow bile" was only the blood serum and not bile at all. The "phlegm" was only the altered serum into which the yellow bile changed by standing. And the atrabilis, or "black bile" of Galen, was only a part of the "crassamentum" (red cell clot) which separated off and assumed a much darker color. The original four humors were, then, only different parts of the blood. For Boerhaave the blood, with all its complex composition, was merely one humor among many, the starting point, so to speak, of the many other body humors.

Blood flowed through the arteries which, beginning with the aorta, became progressively smaller. There was a complicated relationship among the vessels carrying body fluids: several *orders* of circulatory channels, each order smaller than the preceding, from which it derived. The main or parent series began with the aorta, carrying the whole blood. These vessels, subdividing progressively, always remained sufficiently large to accept the red globules. The very small branches of this system (our capillaries) were continuous with the very small veins, which in turn became progressively larger until they formed the venae cavae. These sanguiferous vessels carried *all* the blood components.

But from these vessels there were branches which were too small to admit the red globules yet were of a size adequate for the serous globules. Here, then, was a second circulation, the serous arteries arising from sanguiferous vessels, admitting all particles smaller than the red globules, pursuing a separate course, and flowing continuously into its own set of veins, the serous veins. Using an analogy with electrical circuits we can say that the serous arteries were in parallel with sanguiferous vessels rather than in series. A third set, the lymphatic arteries, were in parallel, so to speak, with the serous vessels. The lymphatic arteries would not accept the serous globules, but all particles of lesser size would readily pass through. The lymphatic arteries emptied into lymphatic veins which transmitted its materials into the serous veins which emptied into sanguiferous veins.

There was abundant evidence to support these teachings, evidence misinterpreted, to be sure, but founded on observation. The sanguiferous vessels—arteries, capillaries, and veins—could be seen by naked-eye inspection or microscopic observation, as in a mesentery or a frog's web. Clearly there was a definite unified network. Similarly, the third order of vessels, the lymphatics, was clearly recognized as a demonstrable and separate system, returning its contents into the great sanguiferous veins. The lymphatics, thought Boerhaave, did not carry either red or yellow globules.

The serous vessels, intermediate between sanguiferous and lymphatic, also had an empirical foundation. We do not normally see any blood vessels on the bulbar conjunctiva. Yet in inflammatory states—conjunctivitis—there are networks of angry red vessels, which disappear as the disease is resolved. This is an inescapable stubborn fact. How to interpret it? Boerhaave's interpretation was that there existed a network of vessels which under normal conditions were *too small* to permit passage of red cells. Under pathological conditions, however, they became enlarged to permit the entrance of red globules. There is a certain delicate logic about the whole concept, which is quite plausible if we shut our minds to other evidence.

Boerhaave, having established three series of vessels based, as he thought, on directly observed evidence, then started to argue by analogy, going from known to unknown. He postulated a continuous series of vessels whose trunks transmitted the larger, the branches the smaller, parts. As the diameters of the branches progressively diminished, the particles they received were progressively more tenuous until the ultimate particles were reached.

He pointed to the fact that there were humors (such as the insensible perspiration) much more "subtle" than lymph, and he assumed there were separate series of vessels to accommodate the progressively more subtle juices.[7]

Before proceeding we must study the vessels as Boerhaave saw them and the fibers which ultimately comprised them. Boerhaave based his concepts on the anatomical data which vascular injection studies furnished. Unfortunately the early studies were technically quite imperfect. Hence Boerhaave, accepting the data at par, built his system on faulty premises. Injection experiments of the period, through technical error, showed that in the large arteries the walls themselves were densely permeated by the injection mass, just as if there were myriads of smaller channels inside the wall. Boerhaave accepted this as a fact and taught that blood vessels were made of membranes which themselves were composed of felted blood vessels. The walls of the larger arteries, in other words, were membranes whose constituent elements were other arteries. Clearly, this process could not go on indefinitely in descending order of magnitude, but somewhere there must be a solid component to form the wall of the smallest or "least" vessel. This "least vessel" was a hypothetical structure, not directly observed. Boerhaave assumed that there were elementary particles—a form of atomic theory—particles which themselves were not further divisible. Two or more elementary particles, joined lengthwise, constituted the simple fiber, the basic solid matter of the body. These "least fibers," joined to each other, made a simple membrane which, when rolled into a tube, comprised the smallest vessel.[8] These smallest vessels, once formed, were thought to become felted into membranes which in turn created still larger vessels, and so on.

All physiology and pathology depended on these basic anatomic data: myriads of vessels of widely differing diameter, through which flowed humoral particles of widely diverse sizes. The body exemplified Newtonian mechanics: particles of differing mass obeyed the laws of motion while flowing through tubes or vessels of various diameters.

Boerhaave had no notion of modern cell theory. He did use the word "cell," especially in such phrases as "cellular membrane," by which he meant the loose connective tissue and its inclosed areolae. What we today call cells—elementary biologic units—were quite unknown, yet Boerhaave vaguely conceived these as special types of vessels. Glands were congeries of especially delicate vessels which elaborated various bodily secretions. A simple gland was formed of a membrane, so convoluted as to include a hollow space. On the membrane were dispersed "small arteries," which "distilled" fluids into the hollow space, whence the contents got discharged through excretory ducts.[9] It is not difficult to modernize this schema. If instead of "small arteries" we think of "secretory cells," we have a fairly satisfactory transformation. Boerhaave's hypothetical "arteries"—for they were inferential and not data of direct observations—performed the secretory function now attributed to cells.

Boerhaave knew a wide range of secretory products—the sebaceous, sweat, and ceruminous secretions; the products of the various salivary glands and those of the entire intestinal tract; pancreatic juice, bile, mucus, milk, urine, semen, all these and others were secreted material, or humors. The glands producing them, though varying in complexity, all consisted of vessels. Circulating blood furnished to glands the raw material from which the special humors were formed. Boerhaave denied specifically that the various humors—acrid bile, viscid mucus, bitter ear-wax—exist as such within the blood. The fluids brought to the gland we can no more equate with the secreted product, he said, than we can call a rough lump of iron a sword or a dagger.[10]

Glands differed one from the other in anatomical complexity, but, more important, they varied in the type of secretory product. How did it happen that one set of vessels would distil mucus out of the blood, while another set would produce bile? Boerhaave struggled earnestly with the problem. First he denied the simple mechanical explanation suggested by Descartes, that the phenomena of secretion depended on pores of various sizes and shapes. Descartes had compared secretory organs to sieves of differing size, straining out those particles whose configurations corresponded to the holes in the sieve. Nor had Boerhaave any sympathy with chemical explanations such as ferments, as claimed by Van Helmont or Paracelsus. The iatrochemists claimed that secretions were made by specific ferments lodged in every gland and vessel. A ferment had the property of changing newly arrived juices into a different nature. Thus, semen was derived from the blood by action of a seminal ferment. This is scarcely a lucid or satisfactory explanation, but at least it pointed in a certain direction. Boerhaave and Van Swieten turned their backs on this and went in the opposite direction.

Boerhaave did not achieve a satisfactory theory. From the same blood the different glandular vessels secreted quite different humors. There was the analogy of plants, which, starting with the same juices of the earth, transformed these into quite different products. He spoke of the "inscrutable mechanism of the animal vessels or the vegetable tubes" which produced this effect.[11] Elsewhere he declared that it was not known how the viscera concocted the humors.[12] He postulated some sort of "action of the solids" on the liquids but was vague about the mechanism.

He did try, however, to correlate differing secretions with certain mechanical and anatomical factors. For example, he claimed that humors varied according to the distance of the arteries from the heart, or the velocity of the blood moving through the vessels, or the ratio of the branch diameter to the parent trunk. The angle that the branch artery made with the parent stem partly regulated the secretions. According to hydraulic principles, the most solid, dense, and spherical particles proceeded in a "right line," while other elements went off laterally. Elasticity of the vessel was important, as were the inertia, gravitation, and "extension and figure" of the blood particles. The latter differed not only in size and shape but in their roughness, angularity, and viscidity. But in addition to velocities and directions and hydraulic principles, Boerhaave also thought that electrical attraction or magnetism might play some role in explaining the variety of secretions.[13]

The nervous system, he taught, was glandular. The nerve fibers were little tubules carrying the most subtle juices of all, which the "fabric" of the cerebral cortex separated from the blood. These juices, once separated, traveled into the medulla and thence into the nerves.[14] The blood, before it reached the brain, was already considerably purified, for as it approached the cranium, the salivary and mucous glands freed it of all the viscid and mucous particles. The various inflections of the carotid also helped to retain the grosser parts of the blood. The very finest, most subtle blood particles passed through series of vessels in the cerebral cortex, became highly attenuated, passed into the medulla, and thence out through the nerves. These transmitted the subtle juices to every part of the body, to every individual point.

The heart, which propels the blood, is muscular. Boerhaave discriminated voluntary from cardiac muscle, as one of the distinctions between the vital and the animal functions. Voluntary movement—part of the animal functions—derived from the cerebrum. The involuntary (or vital) functions, like the motion of the heart, came from the cerebellum. This, he thought, was readily demonstrable in experiments and clinical examples. When the cerebrum was damaged, as by hemorrhage or trauma, there was loss of voluntary motions, while the vital functions—respiration and cardiac actions—continued. If the cerebellum was injured, however, the vital functions would cease. He quoted experiments to "prove" his point.[15] If the facts had been true, his inferences would have been sound, but the crude experimentation of the time produced unreliable data. Boerhaave obviously did not distinguish damage to the cerebellum from injury to the underlying medulla.

How nerve and muscle join was unclear in his mind. Muscle fibers, at least their active and sensitive portions, were to Boerhaave the "very fine *Expansions* of the ultimate extremities of the smallest Nerves,"[16] hollow internally and receiving the very fine and subtle nervous juices derived from the brain. Removing artery, vein, and fat from muscle left the nerve, which furnished the "sensible" and contractile aspects. The muscle was thus *composed* of nerve, plus blood vessels and fat.

As for the heart, its muscle fibers arise from the "eighth" pair of nerves (today recognized as the tenth pair) which entered the heart and dispersed there to constitute the muscular fabric. However, Boerhaave recognized that this was not the whole story and that cardiac muscle differed from skeletal. If the nerves were cut, the movement of the heart weakened but did not cease at once. Compress the nerves, and the animal would survive for a day or two. The conclusion he reached was that there were smaller intercostal nerves which supplied the heart but were not reached by the operation. If the cerebellum or the medulla was compressed, however, the motion of the heart did stop. Boerhaave knew that the heart might continue to beat after it was removed from the body and, when it finally stopped, might be stimulated by a needle prick to further contraction. He concluded that the causes of the contraction were not yet known.[17]

The function of the lungs caused considerable difficulty in his system. Certain facts of the pulmonary circulation were known—the passage of blood from the right side of the heart through the lungs and then back into the left side of the heart. But why this movement took place, or what was the effect of respired air, were not so clear. The lungs were thought to provide a proper agitation and mixing force to the blood which was thereby rendered suitable for circulation and nutrition. Passage through the lung turned the blood red, and blood which was not red (as in chlorosis) was defective. Boerhaave, who knew the major facts of digestion and absorption of chyle through the thoracic duct into the venous circulation, believed that the lungs mixed together—homogenized, so to speak—the blood and the chyle. The "cells" of the lung (the alveoli), distended with air, serve to agitate and dissolve the chyle and humor, mixing them thoroughly with the blood. Within the branches of the pulmonary artery the separate particles of widely differing size acquired a configuration suitable to the various solid and fluid parts of the body.

Just exactly how the fluids interacted with the vessels (the solids) was not entirely clear, but all physiological processes depended on this interaction. The basic mechanism was along the following lines: The heart was the motive power which impelled the fluids through the entire series of vessels. The cardiac impluse, acting on the fluids, dilated the arteries, which, we recall, were of many different orders of magnitude. The arteries by their elasticity pressed back on the distending humors, driving them forward. But at this point there is a hiatus, so profound we might rather call it a chasm or abyss. How did the distention of the vessel and the subsequent reaction bring about a secretion or other change in the humors? Boerhaave could not say; yet somehow he felt that here was the locus of physiological change.

If the proper interaction of solids and fluids constituted health, disease was the faulty interactions of these elements. This dictum of itself is not especially illuminating and is, in fact, quite tautological. Nevertheless, from Boerhaave's formulation we do learn that to analyze disease, that is, to study pathology, we must study first the solids and then the fluids or humors.

Concerning the solids, the most elementary pathologic states involved the simple or constituent fibers which might be either too weak, soft, or lax, or too stiff, firm, or rigid. Such terms are relative. In childhood—and even more so in embryonic life—the fibers, and the organs they compose, were naturally very soft. In old age, they were naturally stiff and rigid. A weak fiber was one whose elemental parts lacked the proper degree of cohesion and which was readily "dissolved" by light motion of the fluids. Obviously infant tissues could not endure the stress to which adult organs were subjected. But this did not constitute disease, which occurred only when the weakness was disproportionate to the age and temperament.

A principal cause of weak fibers—considered a disease state—was imperfect conversion of food. This in turn might result from deficiency of proper digesting humors (humoral cause) or from "sluggish action" of the solids on the liquids (mechanical cause). Eating nourishing food would not necessarily help, since "assimilating power" might be lacking. But if we could increase the "force" with which solid parts acted on the contained fluids, vigor would return. This we might accomplish by giving "steel" dissolved in vegetable acids (iron therapy); by exercise, such as horseback riding or massages; and by sound diet, especially milk, eggs, meat, broths, and wines in moderation.

For extreme debility human milk was especially recommended as was the direct transfer of "effluvia" from the pores of a healthy young person into the pores of the weak and decrepit, which happened when a debilitated person got into the same bed with a young vigorous person. This treatment was given to biblical King David. Van Swieten quoted a more recent case: "A youth at Bologna, labouring under a true marasmus, lived upon the milk of a beautiful young nurse, who lay in the same bed with him, by which means his emaciated body was so well restored, that his friends became apprehensive lest the strength he had gained from her milk should be lost by an unseasonable venery."[18]

Opposite the weak fiber was the rigid or stiff fiber whose particles cohered so tightly that they did not yield to the distending action of the fluids. The vessels which these fibers constituted became narrower, shorter, more resistant to the passage of fluids. To some extent this was a normal aging phenomenon, but excessive exercise or labor accelerated it. To counteract such stiffening, the remedies were a thin watery diet and mealy substances with aqueous, smooth, soft, or oily medicines, all of which tended to soften the fibers.

Excessive laxity—or stiffness—of component fibers might affect one organ but not another. If, for example, the lungs were too "weak," they could not change the chyle into good blood—that is, they could not perform their function. If the liver fibers were too lax, the blood would circulate through it without secreting bile, and a dropsy would result. Or if the stomach was too weak, the whole process of chyle formation was disturbed. Yet one organ could be affected, while the others remained intact.

According to these theories, if the vessel fibers were too weak, they could not act with sufficient force on the distending humors, which consequently would lack the "solidity" imparted by normally reacting vessels.[19] Many serious diseases stemmed from excessive weakness of the viscera. There might result a crudity of the liquids, easy rupture of vessels, effusion of fluids and their corruption, and eventually a phthisis, an empyema, a dropsy, or even an atrophy—all from weak and lax viscera.

In a phthisis the weak vessels eroded, while the extravasated humors corrupted and inflamed the surrounding parts by their "acrimony." In a dropsy the viscera and the vessels were generally weak, and the "transpiring vessels" transmitted excessive fluids into the body cavities, more than the "resorbing veins" could assimilate. To cure these conditions it was not enough to cure symptoms. In a dropsy, for example, it was not enough to draw off the excess water. The underlying cause, the laxity of the fibers, must be remedied. The cure for weak or lax viscera was similar to that for weak or lax fibers and demanded restoring a "lively red color" to the lips or the tongue or gums.

When, however, the viscera were too "strong," owing to too great rigidity of the vessels, the latter contracted and compressed or repelled the liquids. The stiff vessels, dilating with difficulty, resisted the motion of the heart, hindering it from emptying and promoting the formation of "polypuses" (blood clots in heart or vessels, mostly postmortem clots but also some genuine thrombi).

Bloodletting might eliminate the symptoms of excessive rigidity. Vessels that were too firm exerted excessively forceful action on the contained liquids, which rendered the blood dense and compact and predisposed to clotting. Bloodletting would relieve this, since it emptied the vessels and drained off the denser parts of the blood. Appropriate diet would also help, if it included diluting and softening agents. The best diluent for the blood was thought to be warm water, while farinaceous substances helped to soften the solids. Foods and medicines which strengthened lax fibers were to be avoided when the fibers were too rigid.

The blood and humors also had their primary disturbances. Boerhaave, although generally partial to physical explanations, did not by any means neglect chemical theories. In the blood he described particles of water, earth, salt, and oil, which, by quantitative relationship, helped to explain the differences among humors. Too abundant water produced dropsical tumors and weakness. If excessively salt, the juices would be too "acrid," stimulating, and eroding. Too much earth yielded juices that were sluggish and gross, while too much oil produced cold, viscid humors, and the affected person would be fat and inactive.

It is interesting to know how these terms were defined. *Earth* was something so inert that it could neither be dissolved in water nor liquefied through fire. *Salt,* on the other hand, was the generic term for whatever was both liquefied by fire and dissolved in water. *Oil* (or sulfur) melted in fire, and burned, but did not mix with water. *Spirits* mixed with water and also burned with fire.[20] Acids tasted and smelled sour, effervesced when mixed with alkaline salts, and imparted a red color to syrup of violets, ordinarily blue. Alkalies turned the latter green, effervesced with acids, had a sharp taste, liquefied if exposed to moist air, but were quite resistant to fire.

The humors depended on the food eaten. Food initially was thought of as "crude," retaining its own nature in whole or in part until it was thoroughly concocted and changed over into the nature of the body. This concoction depended on the "action of the solids" and on those humors which aid digestion. When these functioned properly, the aliments gradually converted into nutritive substances, conversion taking place in the digestive tract, the absorbing vessels, the lungs, and the blood. The original crudity gradually diminished as concoction occured. If, however, the process was incomplete or defective, the foods retained their own nature, assuming the state that they would have reached outside the body at an equivalent temperature.

This concept is extremely important. Various food stuffs *outside the body,* when exposed to warm, moist air, undergo a variety of changes and corruptions. Some ferment, some turn acid, others get converted into a mushy semiliquid mass, still others become putrid. This we recognize today. Boerhaave maintained that such changes reflected the nature of the particular aliment. But when the food was swallowed, the digestive processes superimposed a new and different series of changes—concoction—which converted the foods into proper nutrients. If the digestive and assimilative processes faltered somewhere along the line, however, the *original sequences native to the aliments* would reassert themselves. The inadequately concocted foods would thus introduce into the blood and humors the varying degrees of residual *crudity.* Clearly, since thorough concoctions required a considerable time span and involved many different viscera, the process could be disturbed at many different points in the temporo-spatial scale. The amount and type of crudity would depend on various temporal and local factors and would allow a great many separate diseases.

Boerhaave distinguished three general humoral changes related to ingested foods. The first he called an "acid acrimony." Many grains, vegetables, and fruit, which when left to themselves would turn acid, produced acids in the body if the digestive processes were not adequate. Acids, thought Boerhaave, were foreign to the normal body. No humor turned acid by itself. Acidity came about when the diet included various acid-forming, mealy, succulent, fresh or fermented, vegetable substances, and at the same time the body lacked "good blood." Or perhaps the texture of fibers and viscera was too weak; or there was a deficiency in "animal motions." From these causes resulted an acid acrimony, producing varied disturbances. For example, in the intestinal tract there might be belching, heartburn, intestinal pain, flatus, spasms, biliary inertia and foul-smelling feces.[21] The blood might be pale; milk, sweat, and saliva might turn acid, followed by itchings, pimples, ulcers, coagulation of milk and even of the blood, irritation of the brain, convulsions, and death.

The cure was obvious. Patients should receive such foods and medicine as counteracted acids: any "animal food" (meat and fish), many leafy vegetables, and various absorbent and alkali powders—"crabs' eyes" especially—which blunted, diluted or changed acids.

The diseases from an acid humor affected especially children; those who took little exercise; "virgins" (by which he means "old maids") who had a "lax habit of body, and are too much given to a sedentary life, if they be of a richer sort; and those that are poor get their living by some sedentary work, and delight too much in drinking warm tea."[22] Poor people who ate little meat and got little exercise were prone to these diseases, as were laborers who worked among acid fumes. We today find it hard to distinguish these diseases from those caused by too lax a fiber, but we realize that similarity in symptoms does not mean similar causation. If the fibers were too weak or the humors too acid, we suppose that the end result might be the same even though the pathogenesis would be very different.

The second type of humoral disturbance concerned excess "gluten." A glutinous substance was one whose parts cohered tenaciously and which could be drawn into threads—what we today might call "mucinous." Some cohesiveness, that is, some degree of gluten, was normally present in the humors, and some fluids, like those bathing the joint cavities, were normally quite glutinous. But excess was pathological. Now certain food substances, particularly starches, formed a glutinous mass when mixed with water. If such aliments were not properly digested, they retained their glutinous qualities, which distorted the humors.

The disturbance took many forms, produced many symptoms. There might be loss of appetite, a sense of fulness, vomiting, indigestion, phlegm in the stomach. Or the blood might be excessively viscid, impairing the circulation and causing pallor. This might be so severe that the blood grew lighter, acquired a cold mucous nature, and produced a swelling in the tissues called "leucophlegmatia." This was quite distinct from a *dropsy,* as readers of Smollett's *Humphry Clinker* will recall.[23] In a dropsy the humors were thin and watery, while the swelling began in the dependent portions. In leucophlegmasias (the more common spelling) the humors were cold and "pituitous," the swelling had a doughy softness and was diffused over the whole body. When there was an excessive gluten, all the bodily functions—the concoctions, secretions, excretions; the vital, natural, and animal motions—were disordered. Various concretions—caseous, mucilagenous, pultaceous, calculous, atheromatous, and others might involve different parts of the body.

The diagnosis was readily made: there was weakness of the solids, a want of good blood, and a history of a glutinous diet. The cure was straightforward: abundant meat and broths to strengthen the viscera, and medicines, such as bile or Venetian soap, to dilute and resolve the gluten.

Steel was also recommended. The physician must not get overenthusiastic lest he expel, along with the morbid glutens, the natural, lubricating, defensive glutinous matter necessary for bodily health.

A third type of humoral disorder arose from a "spontaneous alcaline cause." Certain vegetables, instead of corrupting into either an acid or a viscid matter, produced a "fetid, volatile, fat alcali"[24]—that is, they become putrid. Certain animal foods also had this tendency to putrefaction. When an alkaline putrid acrimony affected the blood, the condition was very grave, for putrefaction was never found in the humors of a sound body. Anything putrid must therefore "be a crudity: for the concocted humors are (normally) bland, neither alcaline nor acid."[25] Heat and moisture increased the corruption and putridness.

Boerhaave, discussing alkaline acrimony, was examining the results of certain bacterial agents of whose very existence he was not aware but whose effects were clear. He knew "putrid corruption" in organic material outside the body. By analogy many severe symptoms in sick patients he attributed to a similar process occurring within the body: stinking breath, vomiting putrid matter, bilious diarrhea, putrid stools and urine, and gangrenes. These he knew well. Ignorant of bacteria, he assigned as causal factor an alkaline acrimony entering the humors through inadequate concoction of various foods.

If we allow his premises, his remedies were very sensible: neutralization or blunting of the alkali by foods which were acid or which turned acid. However, his entire discussion of this topic lacked the assurance and clarity seen elsewhere in his works.

Knowledge of humors and solids considered separately enabled the physician to understand those diseases wherein they acted conjointly. Foremost among these was inflammation. Clinically, many inflammatory states were long recognized and readily identified, but pathologically there was much more obscurity. To explain the observed phenomena Boerhaave elaborated certain theories whose roots went way back to antiquity. These theories, by no means models of lucidity, fairly represent the contemporary medical thinking.

Inflammation, in brief, existed whenever two conditions were present: there must be obstruction of the small arteries and, at the same time, an increased velocity or momentum of the blood stream acting on the obstructing matter. Obstruction alone did not constitute inflammation, which occurred only when there was accompanying heightened velocity of the humors. The increased blood drive ordinarily reflected an increased heart rate, but this forcible agitation of the blood mass might sometimes be only a local phenomenon.

Most commonly the obstruction occurred where the smallest sanguiferous arteries were at their narrowest, just before they communicated with the veins. Normally these channels readily transmitted all the blood components, but, if for any reason the diameter became narrower, or the flowing particles more bulky, then blocking might occur. The particles became impacted within the channels, and the increased forward pressure of the fluids hammered in the impaction, so to speak, creating a firm plug. This process in the small sanguiferous arteries produced a "phlegmon."

Inflammations might also affect the other series of vessels. The serous arteries, for example, which normally excluded red globules, might exhibit inflammations if, by an "error of place," red cells entered the vessels and became firmly wedged by the fluid pressure behind them. The comparable process might occur in any vessel, regardless of size; instead of a red cell, it might be any disproportionately large particle coming from the next larger artery and getting impacted into the lumen. Although this was theoretically possible anywhere among the vascular series, the sanguiferous and serous arteries were most commonly affected. Most of the discussion centered upon the former.

Anything that contracted or diminished the lumen of a sanguiferous artery, so that its diameter was less than a red globule, was capable of producing a phlegmon—pressure, contusion, burns, foreign bodies, and especially various acrids. An acrid was literally something sharp, something which we today would call irritating. There were many acrid substances which produced manifest contractions. Vinegar in the eye was known to induce a strong contraction of the lids. A drop of oil of vitriol applied to the intestines of a living dog caused a very pronounced contraction of the gut.[26] Therefore—and here logic supplements observation—therefore it seemed probable that an acrid substance in the blood stream, reaching the smallest vessel, would produce a similar contraction.

The blood, it was believed, contained many types of acrids. They were all particles "ending in a point, or forming an edge like a knife or sword; whence they are able to gnaw, prick, and cut like so many little wedges."[27] Such was their physical nature, but they were also classified by other properties. They might be acid or alkaline, purulent, ichorous, or putrid. And there are some which represented "a species of contagion of a pestilential kind, which sort of evil has not hitherto been sufficiently explained."[28] That is, some agents, recognized as infectious, were included in the general class of acrids. But for the most part acrids in the blood stream represented an improper concoction of foodstuffs. It was blandly assumed that any action observed *in vitro* would also occur *in vivo;* anything that affected the surface of the body when applied from outside would equally affect the interior of the body when carried by the blood stream.

Disproportion between vessel and contents was thought to take place if anything rendered the blood more coherent or aggregated the particles into masses too large to pass normal channels. It also resulted from loss of the thinner parts of the blood, as well as excessive sweating or watery diarrhea.

Boerhaave had no difficulty correlating the clinical signs of inflammation and these pathogenetic factors. Obstruction produced distention and redness. Distention caused pain. The solids and fluids were compacted—hence the hardness and resistance. Local heat arose from the "attrition" of the fluid particles and the solids against each other. Blood under increased force caused pulsations. Irritation of the various nerve fibers could induce a fever; in Boerhaave's day it was generally accepted that fever could result from pain alone.

An inflammation, once established, could have several possible outcomes. If the obstruction was small, it might be dislodged or perhaps dissolved. In either case the vessels affected maintained their integrity. Then too, there were various medicinal treatments to bring about this result: diluents might help liquefy the obstructing mass; a mild softening diet might relax the fibers, and fomentations might relax the vessels; bleeding was often used to reduce the velocity of the circulation; rest had the same effect. The obstructing material would then pass through the relaxed vessels, and circulation was re-established. It might also happen that, if the humoral impulses pressing on the obstruction were relieved, the plug could be regurgitated, so to speak, back into the larger-calibered vessel. Whether the plug passed backward or forward, the functional continuity of the vessels was restored. This process Boerhaave called "resolution."

If resolution did not occur, the distended vessels might rupture, and their continuity be lost. Humors would be extravasated. These would dissolve and "gently putrefy" to form a "white, thick, glutinous, and unctious humor called *pus*."[29] This he called "suppuration." The pus liquefied those parts which the inflammation had destroyed, and, in Boerhaave's estimation, this was Nature's own way of separating the sound living tissues from those that are devitalized.

If, however, the humors were very "acrimonious" and the motion of the blood very violent, if the obstruction was large and the vessels rigid, then the outcome was likely to be less favorable. The extravasated juices, becoming putrid, might then yield not pus but a spreading "ichor." The affected part would turn an ashen or brown or black color and become insensitive, while the redness, heat, and swelling invaded the adjacent parts. This process was called a "gangrene," representing a limited necrosis which might separate off from adjacent living tissue through suppuration—that is, it might slough. If the gangrenous area extended down to bone, the condition was called a "sphacelus."

There were certain important general aspects of treatment that Boerhaave recommended. The physician should try to soften the obstructing matter and render it fluid or "repel" it back into the larger vessels. Bleeding and purging were very valuable therapeutic aids, because they reduced the volume of fluids and the impetus or violence of the arterial blood. Bleeding released, or lessened, the *vis a tergo* which impacted the obstructing matter so violently into the narrowed vessels. Hence, according to the theories outlined, bleeding was very valuable in allaying inflammation.

But these theories did not gain universal approval. Van Helmont, the arch-heretic, thought bleeding was wasteful, useless, and even harmful. He thought an inflammation (such as a pleurisy) was due to an acid "fixing itself like thorns into the intercostal membrane and vessels."[30] Bleeding did not remove these "spicules" but only wasted the patient's strength. He believed that infections were conquered by specific remedies, a notion that today seems very enlightened. But Van Swieten made a crushingly effective rebuttal: Van Helmont's alleged specifics simply didn't work. When we consider that these specifics included such substances as dried powdered goat's blood (especially if the blood were obtained during castration) or a stag's penis or wild poppy flowers, we are not surprised at the lack of practical, effective results. Van Swieten generalized from a few particular failures to a universal negative. He denied all specific remedies and insisted it was best to diminish the blood's force through bleeding, as he had all along maintained.

A further treatment was the diversion of blood rather than its removal. The main objective was to reduce the force of blood acting on the inflamed part. Bloodletting might accomplish this by diminishing the entire circulation, but a redistribution of blood might effect a comparable result. If, say, there was an inflammation of the shoulder, and if an excess of blood were drawn into the feet, this redistribution of the humors might relieve the vascular force affecting the inflamed area. Counterirritants, so-called, such as warm baths, cupping, blistering, mustard plasters, and other irritating agents, might draw the blood away from the inflammation and so relieve the primary congestion. This therapy had rational theoretical grounds —if we accept Boerhaave's hypotheses.

Another technique for relieving inflammation was establishing a chronic inflammation to divert excessive blood flow away from a primary affected area. A seton, or ligature, passed through a healthy zone, would set up an abscess or chronic inflammation. To cure a severe headache it was recommended by some to pass a seton through the nape of the neck, which, thought its advocates, would divert the excessive blood from the inside of the skull to the outside. Similar theory underlay the use of strong purgatives, which, diminishing the total body fluids, lessened the vascular pressure and thus offered aid in clearing obstructed vessels.

There were, Boerhaave taught, four ways in which an inflammation might terminate. Resolution, suppuration, and gangrene (or sphacelus) we have already mentioned. The fourth was "scirrhus," a condition affecting glandular tissue. When inflammation involved a gland, the excretory ducts might become blocked, the follicles get distended, and a hard indolent tumor result. The most common site was the breast, especially in lactation, when milk could coagulate within the ducts, producing a blockade.

In the more common inflammations the obstructions involved the primary, or afferent, vascular channels. The circulating blood brought sound humors to act on these vessels and their obstructing matter, to promote reaction and to achieve resolution or suppuration. If, however, as in a gland, the blockage was *on the other side* of the secreting membrane (that is, on the excretory side), the obstructing matter was very distal to the sanguiferous arteries. Hence the blood stream could exert only a negligible direct action on the obstructing mass or on its containing vessels. The impedance remained unresolved as a scirrhus, that is, a firm mass which, if well established, only the knife or cautery could remove.

Disorders of the testis furnish very illuminating examples. This organ has a rather small artery which Boerhaave thought subdivided and spread itself into a vast number of small convoluted "branches" forming the substance of the gland. Boerhaave regarded the seminiferous tubules as the direct continuation of the spermatic artery, although the diameter had become diminished to extremely minute size. If there was any inflammation in these secreting vessels, the matter would be outside the direct course of the circulation. The spermatic artery, of small caliber even at its maximum, could not furnish enough blood or good juices to resolve or concoct the obstructive matter. Hence a scirrhus of the testis might be extremely refractory. On the other hand a gonorrhea involving the urethra might block the excretory vessels of the testis and produce a swelling of that organ. This was much easier to treat, since, when the urethral inflammation was cured, the testicular obstruction would automatically be relieved.[31] The ease of cure depended on how readily the obstruction could be overcome. The examples imply a distinction between a secondary and a primary involvement of a gland, between an ascending and a hematogenous affection.

Of course, neither Boerhaave nor Van Swieten distinguished an infectious from a neoplastic process in our modern sense. A scirrhus was any hard swelling which was not especially painful. This is an extremely broad definition embracing a tremendous number of diverse conditions. Boerhaave and Van Swieten discussed many examples which today we designate quite differently. For example, there were various benign and malignant neoplasms (provided that they were painless—an important proviso). From the discussions we can readily identify benign tumors, such as leiomyomas of the uterus, and malignant ones, such as carcinoma of the breast or of the esophagus or of the pancreas. There was a wide array of breast disorders, including mastitis and fibrocystic disease. Enlarged lymph nodes (lymph "glands") were also scirrhi, in which the etiology might be granulomatous infections, metastatic carcinomas, or primary tumors. Fibrosis of organs, with or without calcification, as cirrhosis of the liver or atrophy of the spleen, were likewise scirrhi, even though there was no enlargement. There was a whole range of postinflammatory lesions with fibrosis and fat necrosis. These, exemplifying scirrhus, were among the common examples with which Boerhaave was quite familiar but which, of course, he interpreted quite differently from our present concepts.

His notions of causation reflected his general attitude. Inflammation as a cause of scirrhus we have already mentioned. Other related causes, which he listed separately, include a stagnation and coagulation of milk, a contusion, a violent attrition, an "anthrax, a bubo, or an ulcer closed before its time."[32] Those are all related to the general concepts of inflammation, with obstruction, failure of resolution, and fibrosis playing the major roles. Even to Boerhaave, however, these did not explain all the admitted forms of scirrhus; so he added "an atrabiliary matter of the blood and bile," that is, a primary humoral disorder. This concept "explained" certain spontaneous tumors and yet kept the basic tenet of obstruction. Van Swieten in his commentary pointed out that long-continued passions (or violent exercise) might destroy the most fluid parts of the blood. Hence, "from the oily and earthy parts of the blood combined together, an impervious tenacity is produced in the blood, by which it passes with more difficulty. . . . this tenacious matter, so extremely apt to form obstructions, is called *atra-bilis*."[33] This is "most apt to produce scirrhosities." Then, as clinching evidence, he declared that "scirrhosities are most frequently found in those who are naturally inclined to an atrabiliary habit of body."

At any rate, Boerhaave appreciated that many scirrhi had fairly demonstrable causes, while for others arising spontaneously he invented various hypothetical explanations. These, when we eliminate all the verbal juggling, add up simply to this: Tumors (scirrhi) arise in people whose humoral composition predisposes them to tumors. Not perhaps very profound, but not too different from much of our twentieth-century teaching.

A scirrhus (all different tyes lumped together) was considered curable only in its very early stages and then only with utmost difficulty, while a confirmed established scirrhus was incurable, except occasionally by surgery. The great danger was that scirrhus might lead to cancer. The differential feature distinguishing the two was *pain.* A scirrhus, originally painless, might acquire some irritative destructive property (acrimony) which caused pain. By reverse reasoning, development of pain in a scirrhus indicated a cancerous transformation. Itching, pain, and heat were the first signs indicating an "occult" cancer, while the later or florid stage was ulcerated. An ulcerated cancer was a hopeless condition, and Boerhaave and Van Swieten gave gruesome descriptions.[34]

Regarding causation, there were some sound observations which, however, led to misinterpretations. It was well known that if external irritation were applied to a scirrhus, an ulcerating carcinoma often developed. Therefore, part of the treatment of a scirrhus was avoidance of irritation. Furthermore, one cause of the worsening of a scirrhus was an increased velocity of the blood. "Acrids" taken internally might increase the circulation and hence were to be avoided.

Cancer suffered a "corruption" and acquired corroding properties. Our authors, apparently, held that the invasive destructive action resulted from the ulceration. In a cancer the good or favorable arterial humors could not penetrate adequately. New juices reaching the cancer were speedily corrupted into an "acrid sanies." This matter was so "sharp" that it could not have pre-existed in the blood but must have been generated in the diseased part. The sanious discharge from a cancer might, so Van Swieten averred, corrode the bed linen as would nitric acid.[35] It is no wonder, therefore, that this sharp matter should corrode and eat away parts of the body.

But what *is* this corruption, and how does it differ from other forms? Just how does it corrode? Boerhaave would answer: The cancer corrodes because it has developed an acrimony. And how does that produce the effect? Well, he could point to the action of a strong acid whose destructive effect on the tissues was obvious: all you need do is look and be convinced. But, you might object, there is no *aqua fortis* or oil of vitriol produced in a cancer. True enough, but there is a destructive action *similar* to that which strong acids produce. And so we revolve in a circle, juggling a few words around, arguing by similarity, but actually not progressing beyond this simple observable fact: cancers do erode and spread and destroy.

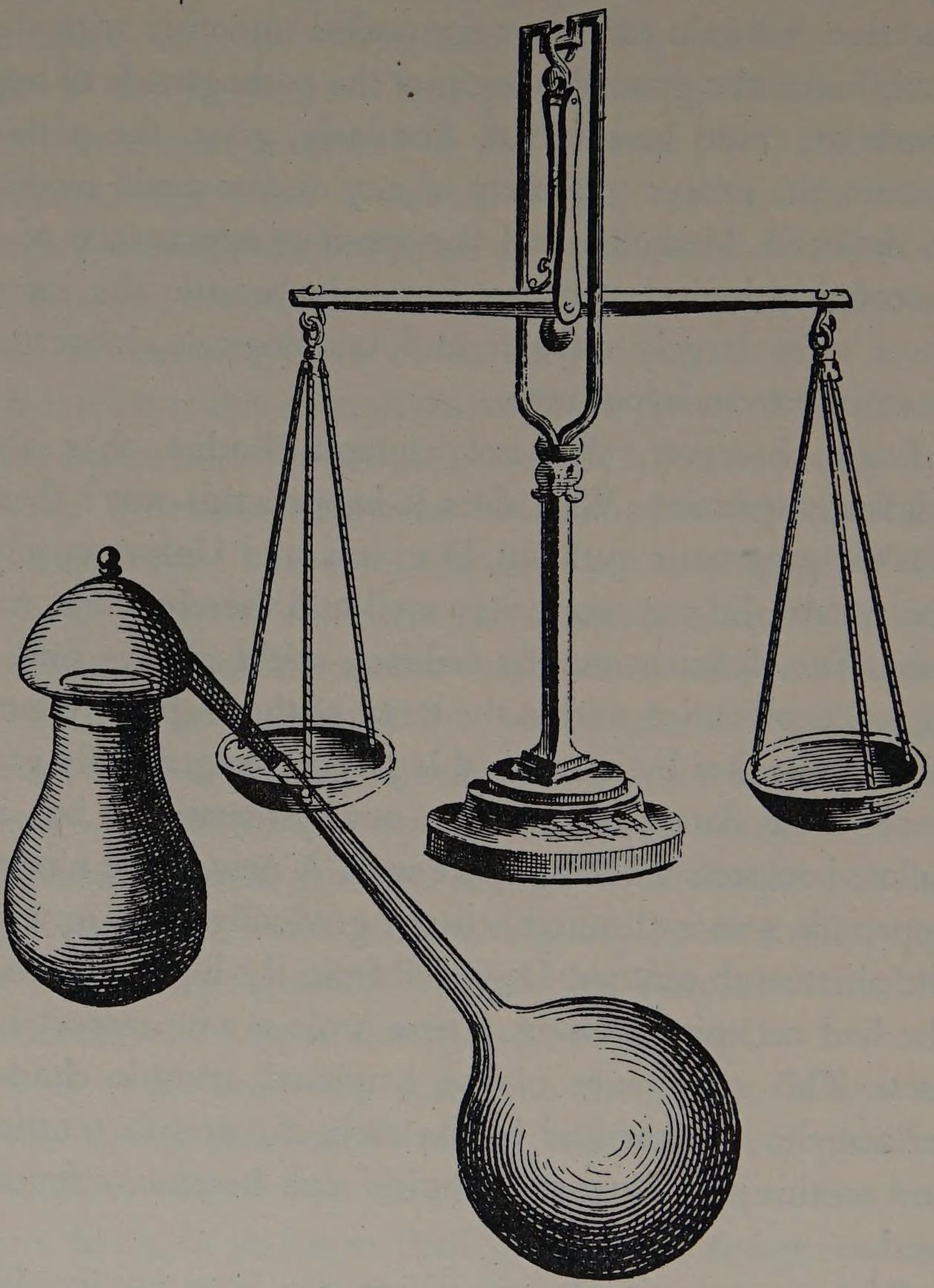
And as for cure—the less done the better. When an occult cancer developed from a scirrhus, those patients would live longest who were left alone. Any treatment was more likely to do harm than good. This, recognized by Hippocrates, was still appropriate in the eighteenth century.

The topics so far discussed, the elementary anatomic framework and the basic physio-pathologic principles, outline the foundation of Boerhaave's system. Innumerable other aspects remain, including all of present-day "special pathology," but further analysis would suffer from diminishing returns.

Boerhaave was quite consistent as befits a good system-maker. He elaborated a relatively small number of principles, variously combined, into a complete, well-rounded doctrine, wherein each part dovetailed smoothly with the next. From the general principles the pathogenesis of any condition could be inferred. Similarly, given the pathogenesis, the proper treatment of any disease could readily be deduced. Medicine had the specious appearance of a thoroughly logical structure but only because the inferences were largely circular and tautological, elaborated from unproven hypotheses.

Logic, however, was not enough. Rather than the scholastic question, Why does it happen this way? there was the pragmatic question, Does it work? Unfortunately, the system did not work very well and therefore was not true. The deficiencies, the failures, could always find a logical explanation within the terms of the original system. New researches by innumerable physicians gradually produced new data, fragmentary, not systematized, but in isolated respects working quite well. A new, more modest approach, a more limited activity, gradually came in with the nineteenth century. Logic fell from the high place that she had occupied so long. There arose a new respect for *facts.* This resurgence of the empirical attitude characterized the best method in the early nineteenth century, and on this pragmatic and empiric rock Boerhaave foundered.

Of course, no simple empiricism can long survive. We cannot long dispense with systems and theories. We cannot here trace the rise and fall of successive new systems. But when we can thoroughly dissect a dead system such as Boerhaave's, we can appreciate both the merits and the faults, and so we can approach our present-day systems with new insight.



IV

Hermann Boerhaave, Scientist

The great Sylvius (1614-72), so it is related, once had occasion to treat an epidemic fever but was so unsuccessful that the majority of the patients died. He wrote learnedly about the disease, claiming that his treatment was sound, his remedies most appropriate, but that God had withheld His blessing in order to punish the men and women for their sins.[1] Now a practicing physician might agree that the remedies were truly appropriate; a theologian might agree that the patients' grievous sins richly deserved punishment; but what would the scientist say? Surely, a scientist, before assenting to Sylvius' proposition, would demand evidence. Science, in fact, has recently been aptly characterized as "knowledge peculiarly concerned with evidence."[2] Diligence, ingenuity, and keen observation will avail little unless the information thereby gained can be suitably evaluated. Such critical ability is an indispensable tool of science. It is not enough merely to allege a fact or assert a theory. The real scientist must also ask, How do you know? Why do you say it? How good is your evidence?

All too rarely do physicians ask such questions, but from time to time someone will analyze the basis of medical knowledge and become more or less self-conscious about the so-called "scientific method." Boerhaave was such a one, and his formulations deserve study.

The particular epidemic in which Sylvius invoked the Deity may never have come to Boerhaave's attention, but theological excuses and explanations calling on the Deity were certainly familiar enough. Francis Bacon had inveighed against them, as had all subsequent empiricists. God did not come within the scientist's realm of investigation. God, the primary cause, was a metaphysical concept which, by definition, was beyond the scope of physics. Boerhaave declared specifically that ultimate metaphysical causes "are neither possible, useful, or necessary to be investigated by a physician." When seeking the so-called primary causes, we go too far beyond concrete evidence and then we "always meet with God in our Search." We come to the complete impasse—"God made it so"—which effectively puts a stop to any further questions.[3] Hence, Boerhaave felt that scientists, instead of seeking or invoking first causes, should confine themselves to those causes which alone are subject to physical and scientific investigations.

A physician, said Boerhaave, should base his knowledge on the data of anatomy, chemistry, mechanics, and "experimental philosophy." There are two sources of knowledge—and repeatedly we find these two touchstones mentioned—first, facts and experiments, and second, reasoning founded upon them. These two are the way to truth.[4]

"Experiment" is not used in our own contemporary sense but rather is synonymous with "fact" (that blessed word which has provoked so many centuries of dispute). They both refer to something objective and independent, to "an appearance obvious to the Senses of the Enquirer; our mind adds nothing to the Appearance, but barely the Perception of it."[5] This naïve realism assumes that "facts" are "out there" and that the mind, perceiving them, does not by that act of perception or experience change them in any way, any more than a mirror changes an object which it reflects. A fact (or experiment, in his sense) "carries Conviction along with it, and is indisputable; nothing being more certain than Demonstration from Experience."[6]

The perception or experiencing of a fact is a *passive* operation, carrying absolute certainty but not carrying it very far. When, however, the mind starts to "reason" about facts, it is *active*. It extends; it goes from the known to the unknown; it explains and relates. And proper reasoning can be just as true as any fact, can give just as much certainty or assurance. The classical examples of certainty derived through reasoning is the whole field of mathematical demonstration. We start with general principles and proceed step-wise, "so that no Body who admits the general Principles, can deny their Assent."[7] This is the deductive method. Once we have the first principles, conclusions necessarily follow, with absolute certainty if the logic is correct. The theorems of Euclidean geometry, so familiar to high school students, exemplify this method.

But mathematical reasoning, although sometimes applicable to medicine, does not cover the entire ground. We need to draw conclusions regarding causes which occurred in the past and effects which lie in the future. We need to take generalizations and to extend the information gained at one time to cover events at a different time. In other words we must make inductive leaps, drawing inferences which cannot have the certainty of true deductive conclusions.

Now this is the very crux of scientific thinking. When do we know that our inferences are sound or "just" or true? Boerhaave's answer seemed to be that an inference is true when it is "so evident as to compel every reasonable Person, skilled in his Profession, to allow it for true," or when it follows from the facts "so evidently, that no skillful or unprejudic'd Person can refuse their Assent."[8] It was Descartes' "clear and distinct idea" all over again when accepted as such by competent open-minded observers. This seems to have been his criterion of inductive truth. Anything short of this sank to the level of mere opinion, which is fallible.

The doctrines propounded by various medical sects were only opinion which lacked the force either of fact or sound reasoning. Boerhaave gave as an example the burning "heat" which an inflammatory fever induces in the patient. That feverish patients feel hot is a fact, indisputable. But the inferences regarding its cause were mere opinions, that is, speculative hypotheses. Galen thought the feverish heat arose from excessive bile; the iatrochemists, from excessive sulfur in the blood. Van Helmont blamed an Archaeus. All these Boerhaave disdained as mere conjecture, or "Hypothesis." He recognized that inferences concerning causes were much more "dubious and uncertain" than direct experience which is truly convincing.[9]

Now Boerhaave, as a leading physician who extended the frontiers of medicine, concerned himself with evidence, demonstrations, proofs, causes, inferences, and conclusions, and with their critical evaluation. His thinking showed both the merits and the defects of early eighteenth-century science. It is instructive to see how he wrestled with various problems of medical science and of scientific method. We can examine some of his inferences and conclusions to note how well he followed sound inductive reasoning and to what degree he committed the same errors he criticized in others.

Boerhaave could exercise a very sharp critical judgment, especially toward the works of others. Moreover, he was on occasion a capable experimenter, often exhibiting praiseworthy scientific acumen. For example, the end of the seventeenth century witnessed great controversy over the nature of digestion and the digestive juices. Chemistry was popular but poorly understood and its concepts vague. Sylvius had claimed that the bile was alkaline, the pancreatic juice acid, and that the mixture of the two in the duodenum produced an "effervescence" characteristic of acid-alkaline reactions. There was a viogorous controversy regarding this point, much of it verbal, some of it experimental. Although the simple *in vitro* mixture of these two juices produced no effervescence, such evidence was not conclusive. Schuyl, a professor at Leiden, performed *in vivo* experiments. He ligated the duodenum, both above and below the entrance of biliary and pancreatic ducts, and then kept the dog alive with the closed loop intact. A few hours later, when the animal was opened, "the intestine between the ligatures will be found tense, dilated, and hot; and upon making an incision in it, there is discharged a froth and great stench; so that it is thus manifest by ocular demonstration, that the bile and pancreatic juice do effervesce upon mixing in the animal."[10] Thus claimed Schuyl, offering his experiment to support the claim.

But alas for experimental pitfalls! What Schuyl held up as experimental fact was only a tissue of fallacious inferences. Under the conditions of the experiment there would be, unquestionably, a froth and a stench, but did this prove the point under dispute? Not at all. Boerhaave was able to demolish his opponents through simple but strict scientific rigor. He pointed out that if ligatures were placed around the intestine *below* the entrance of biliary and pancreatic ducts, the same effervescent froth would appear. Clearly, since both bile and pancreatic juice had been excluded and did not take part in the reaction, the effervescence should be attributed to something else, such as "the elastic Air generated by the Fermentations of . . . stagnant chyle."[11] Furthermore, when in the absence of any ligatures the pancreatic juice and the bile were mixed in the living animal, they united smoothly without the least effervescence. In essence Boerhaave added both positive and negative controls to Schuyl's experiment. These proved that although "ocular demonstration" of effervescence did indeed occur, it was due to factors quite other than those originally invoked. A better mastery of the scientific method we could not demand.

Boerhaave was in general fairly inquisitive. Although he had the utmost respect for tradition, he also wanted to know facts. Usually he was satisfied by the evidence that others might offer, but when this seemed insufficient or inconclusive he often undertook his own experiments to determine what the facts actually were. Throughout his works we find many scattered references to experiments. To choose a few random examples: he observed at first hand the digestive effects that saliva exerted on bread. He knew that bread swallowed whole without chewing and then vomited up again after a few hours showed little or no change. But if the bread was well chewed and mixed with saliva, the stomach contained a thick white fluid like chyle. At one time he thought that the stomach could "dissolve" hard materials. In order to satisfy himself he fed dogs bones, ligaments, and solid or tough materials and then noted that these were excreted unchanged, except, perhaps for the "succulent parts."[12]

Discussing urine, Boerhaave mentioned the clinical observations of Van Helmont and others and described various experiments on chemical reactions, relations of fluid intake and urine production, or of exercise and urine sediment. One particular experiment deserves mention. There was a claim "by a celebrated professor of chemistry" that blood serum was to be found in the urine. Boerhaave, to test this statement, examined over a hundred pints of urine but could never find anything "which would congeal like the white of an egg by the boiling of water." He collected a whole cask of urine, whose weight he reduced by evaporation from one hundred pounds to one pound. But, he said, "I could never find anything in the urine which would cut like a boiled egg."[13]

Obviously he lacked delicate tests for protein, and his principles of sampling may have been faulty, but his basic respect for facts and the experimental approach we cannot criticize. His techniques were imperfect, but even with inadequate techniques a physician may be an excellent scientist, if he remains sufficiently critical.

From the perspective of 250 years it is easy to point to Boerhaave's errors. But errors may be due sometimes to insufficient data, sometimes to uncritical or fallacious reasoning. A man's scientific status depends more on his critical judgment than on his factual information. There are two general areas where the scientist is called on to exercise judgment: in "factual" or empirical studies, where he aims to establish correlations between observed events; and in "conceptual" or rationalistic studies, where he tries to explain the correlations. The scientist may note that events A and B seem to go together, that when you have A you can expect B to follow, and he concerns himself with establishing this correlation as a fact. The rationalist seeks to know *why* it is so, that is, to supply the missing links, the intermediate steps which, hidden from direct observation, will furnish the connection whereby B follows A. Sometimes the rationalist will discover new observed data, such as A′ and A″, B′ and B″, which interpose between the two original terms. But regardless of how many sensory observations connect A and B, there will always be a conceptual framework to tie the observations together. These are the theoretical concepts with which science deals. How critical was Boerhaave in his empirical or conceptual activities?

Boerhaave made numerous valid correlations, which is another way of saying that he described many definite facts. For example, quite striking (although not original) is his analysis of certain forms of what we now call anemia. He was well acquainted with the now vanished syndrome, chlorosis. He described the symptoms in young girls, the dyspnea on exertion, palpitations, swooning, slow pulse at rest and marked quickening on exertion, poor appetite.[14] He knew that the cure lay in administering "filings of steel," which would bring back a red color to the pale lips and vigor to the patient. This, of course, is primitive iron therapy. The correlation is sound: in all cases where there is a certain group of disease findings (which today we call iron-deficiency anemia), give iron and the disease will disappear. It is not merely that this happened to Patient Hilda and to Patient Gretchen, who are only particular instances. The scientific acumen enters in realizing that *any* patient who shows these findings will react in the same way. This is a scientific observation, a valid general correlation. In so far as there are no, or only very few, exceptions, that is, if the correlation is reliable and useful for prediction, it is good science. If, however, the correlation is hedged with exceptions and often fails, if prediction or prognosis are very questionable, it is bad science.

Now it is very desirable to have some explanation at hand, some reasons why the "steel" which Gretchen swallowed should restore her health. For Boerhaave, the explanatory link which he utilized to connect and explain the observations took the form of excessive "gluten." Get rid of this excess through medication, thought Boerhaave, and the patient will recover. Today this is not acceptable; we prefer explanations involving hemoglobin formation. But if you know nothing about hemoglobin, you will, as a rationalist, use some other concepts to tie together your observed data. The eighteenth century abounded in excellent scientific observations on the empirical level which are still valid today, even though the reasons given at that time are now completely discarded.

A sound correlation, like the administration of iron and the cure of certain anemias, will endure, although the explanation offered at that time proved faulty. Quite different is the situation where the original data are incorrect, the alleged correlation faulty. Then the reasoning may be logical and cogent but, if based on incorrect premises, it is incorrect. Let us note an example where Boerhaave's original data were false. The problem concerned the functions of the brain. The scientific error lay not in having wrong data but in not using adequate controls. Thus, Boerhaave recognized the distinction between voluntary (or skeletal) muscle and cardiac (or involuntary) muscle. He knew that there was a relationship between nerve tissue and muscular contractions. To clarify some of the phenomena, he quoted experiments by Duverney, who compressed the "brain" (that is, the cerebrum) and produced thereby a paralysis of voluntary muscles. The animal did not die from this injury; yet, if the cerebellum was compressed, the animal would die. Accepting these as facts, Boerhaave reasoned as follows:

> We manifestly perceive two springs of motion in the muscles: since the voluntary muscles which are directed by the influence of the mind, become paralytic by compressing the brain, from whence, therefore their nerves must be derived; but the involuntary muscles, as the heart, stomach, intestine, and those of expiration, continue their action while the brain is compressed, but are all immediately rendered paralytic, when the cerebellum itself is injured; and therefore these last must derive their moving powers from the cerebellum.[15]

He did not believe that any nerves arose directly from the cerebellum itself but held that the cerebellum originated the moving powers, which were transmitted through the medulla and the nerves arising therefrom.

If the data were true, this would be fine reasoning: There are two different types of muscle, each under the control of nerve centers. When one part of the brain (the cerebrum) is injured, the voluntary muscles become paralyzed, but the other type, the involuntary, does not. The latter, however, become paralyzed when a different part of the brain, the cerebellum, is injured. There thus appears, at first glance, to be a one-to-one correlation between the integrity of certain brain areas and the functions of different muscle types.

If the original experimental observations were correct, that is, if the asserted correlation were reproducible under varied controlled conditions, the conclusion would be elegant and cogent. But the original data were wrong. There were no adequate controls, nor did Boerhaave realize how flimsy was the evidence without such controls. Had Duverney made experiments wherein the cerebellum was kept intact but the brain stem was injured, or had he kept the medulla intact while damaging the cerebellum alone, he would have had different results. But perhaps the times did not afford the requisite techniques. Experimental brain surgery was extremely crude. While gross destruction or massive hemorrhage could be recognized, there was no way to identify small or subtle damage. The interesting feature is not that Duverney was wrong but that Boerhaave should have accepted the evidence without more critical evaluation. When Boerhaave stated that the vital functions arose in the cerebellum, he believed that he grounded his doctrine not on "hypothesis" but on a solid experimental basis. In this instance he accepted someone else's statement at face value and upon it built a theoretical superstructure without considering the questions, Was the evidence credible? Was it adequately controlled? It is indeed striking to compare his gullibility here with his critical acumen regarding the pancreatic juice.

A different type of error illustrates a different sort of uncritical acceptance. Boerhaave was well acquainted with many automatic bodily defense mechanisms. Thus, he pointed out that if a grain of sand fell into the eye, the eyelids quickly and strongly contracted "and the eye by that means thrust further into the orbit; whence the large lacrymal gland being compressed by it, a stream of tears follows to wash out the sand and abate the inflammation.[16] This statement represents a curious blend of fact and fiction; in part there is a valid empirical correlation. Very often a grain of sand or other foreign body will make the eyelids contract strongly and will also provoke a flow of tears—not always, but often enough to establish the correlation. Then the question arises, Why? What are the concepts that tie together these two separate events? What warrants asserting a cause-and-effect connection? Boerhaave, as a rationalist, wanted to integrate the several data by some conceptual schema which would make them part of a total system. Given the simple factual statement, "A foreign body in the eye provokes tears," he invoked certain intermediate steps. First, there was the contraction of the eyelid, which he could see and which was a further sense datum. Then he also invoked the *mechanical compression* of the tear sac, something which he could not see. His readers probably did not even stop to think that the alleged compression was in no sense a fact but only a hypothesis interjected among the foreign body, the contraction of the lids, and the tears, in order to tie this sequence together.

Boerhaave knew nothing about reflex arcs but, influenced by seventeenth-century science, he did know quite a lot about simple mechanics. Mechanical principles were "in the air," forming an intellectual climate which shaped learned judgments and vulgar prejudices. Such prevailing concepts pushed their way silently into current thinking. In Boerhaave's day a mechanical explanation came so "naturally" to mind that it probably never even occurred to him, or to his readers, to seek positive confirmatory evidence. Nor did he even seek controls. It would have been quite simple, had he thought of it, to squeeze the lids tightly together or to compress the eyeball digitally with the lids closed to see if a flow of tears resulted. But this he did not do.

To Boerhaave simple mechanical principles furnished very convenient working hypotheses, quite adequate to explain many physiological processes. In their favor was vague fashion. Against them was no positive evidence and no outspoken objection. Remaining unchallenged, the proffered explanations stood by default, without any critical examination. Boerhaave did not distinguish a tentative or trial hypothesis from one more firmly grounded, nor did he realize that tentative explanations needed positive confirmation to become truly valid. In brief, he did not distinguish between *what might conceivably be correct* and *what is demonstrated by strong and direct positive evidence to be very probably correct.* He adopted a hypothesis when it was merely a possibility. He did not seek probability. It is indeed common to assume a hypothesis correct if there is no definite positive evidence against it. But good science demands that any hypothesis must be demonstrated to be correct by direct positive evidence in its favor and by adequate controls which eliminate coincidence.

This error—uncritically taking for granted some plausible explanation more or less harmonious with the current teaching—is not a parochial matter. It is not restricted to a single country or a single century but is quite as prevalent today as it was two or more centuries ago.

The basis of inductive reasoning—medical and nonmedical alike—is *analogy*. We note similarities. And then we expect that what is true of one thing will be equally true of others which are similar to the first. The earliest medical practice supposedly arose in this way. There were chance observations that some herb or drink or manipulation eased some disorder. The next time that disorder occurred the same remedy might be tried. What helped one patient might fairly be expected to help another, if both had similar diseases. If the treatment was again successful, a pattern might be set up and a mode of therapy established. Here we have an initial correlation, based on analogy. Further experience could strengthen the analogy still more or, contrarily, prove the supposed correlation illusory. Unusual circumstances, whose significance might impress an astute observer, could furnish crucial data for scientific progress.

Boerhaave described how, in this way, certain therapeutic means became popular, as, for example, phlebotomy as a therapy for fevers. Thus, "pleurisy" was long known as a fatal disease which "usually ended in a fatal mortification within the space of three days." But a patient with pleurisy who had a severe wound, from which he bled profusely until he fainted, was noted to recover "whereas others in the like Disease perish'd at the same time."[17] Nature, quite by accident, provided a controlled experiment: of numerous patients with the same disease the one with chance hemorrhage got well. Profiting from this gratuitous advice of Nature, ancient physicians suggested that artificial hemorrhage—bloodletting—might benefit fevers. Further experience helped indicate when bleeding was especially advantageous, when not.

Reasoning by analogy may be far more sophisticated and yet follow the same patterns. A is like B. What is true of A should also be true of B. If a patient has pain in the chest and fever, difficult respirations not relieved by expectoration or bleeding or other remedies, and if he should die, autopsy might show a mass of purulent matter compressing the lungs—an empyema. Thus we learn to connect clinical appearances and causes. Then another patient who shows the same clinical appearance and follows the same course may fairly be expected to have the same condition and the same unfavorable outcome.[18] Here Boerhaave appreciated the scientific value of the autopsy. And he rightly implied that analogy is the entire basis of prognostics, which played such a large part in early medicine.

Improper analogy leads to error. An interesting example concerns the properties of alcohol. Boerhaave knew that alcohol—spirits—would coagulate certain material such as egg white or blood. Furthermore, he knew that animal tissues kept in alcohol became progressively harder as well as contracted. Hence he reasoned that when the fibers of the living body were too lax or soft, judicious use of wine could harden or stiffen them to proper consistency. Moreover, to make the analogy more stringent, he noted that drunkards, at autopsy, were seen to have viscera "scirrhous and indurated." Hence he concluded, very logically, that "we ought to use the utmost caution, in giving fermented spirits. . . . they inspissate the liquids and contract the solids, if used imprudently."[19] This is clearly a fine chain of reasoning based on analogy. Alcohol *in vitro* would coagulate tissues. There was strong evidence that it behaved the same way *in vivo,* since excessive liquor intake led to shrunken, hardened viscera. By logical inference, moderate amounts of spirits, taken internally, should provide an optimum degree of "hardening" when the fibers were too soft (assuming from other evidence the existence of such fibers and their variable consistency).

Boerhaave's generalization that alcohol shortens solids and coagulates humors, both inside and outside the body, will not hold. It completely neglects the differences between *in vivo* and *in vitro.* The similarities he noticed; the differences escaped him. He knew nothing about liver cells or fibroblasts, or specific dietary deficiencies, vitamins, or cellular metabolism. Because he did not know these things, he could not assay their relevance. His own explanation, his own analogy, stood by default, from lack of contrary evidence. This emphasizes a basic logical principle which no one ever seems to mention: mere absence of contrary evidence does not bestow any cogency whatever on a given assertion.

Boerhaave's system exhibited fine symmetry. Sometimes, he believed, the body fibers were too hard or stiff so that restoration to health required some softening action. The remedy he advised he again arrived at by analogy: Watery and fatty foods would soften and weaken the fibers. There was ample *in vitro* evidence. He pointed to the fact that warm moisture would soften staghorn and that animal hides, however stiff, became softer if treated with oil. Hence, by analogy, he recommended the same remedies, taken internally, for the living body, neglecting all the differences between shoe leather and live human viscera.

If two cases are essentially or really similar, what works in the one should also work in the other. The problem still remains whether there *is* an essential similarity. Today, just as much as 250 years ago, we face the question, Does a test-tube experiment really apply to the living body? Do observations on guinea pigs or rabbits or monkeys also apply to man? It takes no great genius to realize that a test tube is not alive and that a rabbit is not a man. Can we nevertheless establish a real similarity which would make the analogy cogent? Or do we merely fail to note relevant differences, accepting the analogy through default of contrary evidence?

Occasionally, Boerhaave himself showed an awareness of this problem. When discussing the action of the nervous system, he postulated a very subtle humor, secreted in the brain, which flowed through the very fine tubules that were the nerves. We have already seen how he explained muscular contraction as a function of nerve activity which for voluntary muscle arose in the cerebrum but for involuntary muscle, in the cerebellum. Now, he knew that certain amphibia and reptiles behaved somewhat differently. The eel, for example, when cut into several pieces, still continued to exhibit local movement in the parts for a long time. This fact, we might think, would invalidate the concept of humors flowing from the brain to the muscles or viscera. But Boerhaave drew the inference "that the Fabric of the nerves in cold amphibious animals is different from that of the Nerves in Quadrupeds and hot animals: so that no Argument of Force can be thence drawn to make any conclusion with regard of the human Body."[20] In other words, he says that the analogy is faulty—an eel is not a mammal, let alone a man, and no valid comparison is possible. The differences between eel and man so outweigh the similarities that we cannot argue from the one species to the other.

There is an important psychological aspect. Boerhaave started out with certain theories. If new observations such as those on the eels did not fit in with the theory, he found good reasons why the new observations were not applicable. He then stressed the differences between species, rather than the similarities, and concluded that no analogy could be drawn. Hence, he considered the observations on eels true but irrelevant. On the other hand, if new observations did fit in with his doctrines, he was perfectly willing to argue by analogy, neglecting the differences and stressing the similarities. What we call critical acumen depends on our emotions as well as on our intellects. We tend to accept as evidence what we want to accept. What we do not want to accept we can usually find adequate reasons for rejecting.

In any analogy the points of similarity might be inconsequential, and the points of difference, which might be unknown or, if known, ignored, could prove far more important. Boerhaave realized that it was wrong to generalize too hastily or to carry over the results from one case to another. Treatment appropriate for a given disease would not cure a patient who suffered from some different affliction mistakenly thought to be the same disorder. And he said, specifically, "When we apply the Phaenomena of one Body to account for the Appearance of another . . . we are frequently deceived";[21] this also happens when we faultily assume "that Propositions deduced from a few Experiments will hold true throughout Bodies in general."[22] There is the clear recognition that conclusions, inferences, may outrun the evidence. When this occurs, we have error; when the conclusions, on the other hand, are valid, the reasoning is "exact" or "just."

This formulation actually does not carry us very far forward. It merely states that sometimes we make mistakes but at other times we infer correctly, a conclusion which as a general principle is perfectly obvious. The important thing is to tell *in advance* when our inferences will prove correct, or probably so; to know when they should inspire a high degree of confidence and when they should be considered doubtful or questionable. This information Boerhaave did not explicitly give. What are the criteria for sound inductive reasoning? Such criteria, of course, are best known today through John Stuart Mill's *Canons of Induction,* published in the mid-nineteenth century. But they were clearly foreshadowed by Francis Bacon and David Hume in the seventeenth and eighteenth centuries, respectively. Boerhaave did not clearly or explicitly grasp the principles of sound induction; yet he glimpsed vaguely the problems involved and groped in hesitating fashion for those solutions which have evaded even more brilliant scientists in later centuries.

How do we know when our inferences are probably correct? It is not enough to assert loudly, or triumphantly to call in learned authority. Nor is it enough merely to start with experience and then draw our own conclusions. A proposition may, we have seen, be "manifest by ocular demonstration" and yet be wrong. The significant problem is, Can we translate our inferences forward into future experience and feel confidence that they will meet that test? This goal of modern scientific method Boerhaave did not fully recognize. Nevertheless, he did have some dim insight.

Scientists trying to determine what one thing has to do with another customarily use the phrase "cause and effect," which, however, exhibits such manifold and varied meanings that it proves a veritable quicksand. Boerhaave had his own notions on the subject, namely, that "by experience we learn, that one Thing exists, and that if the Existence of another Thing is conceived to follow of Necessity from the former, then the first is termed the Cause, and the latter the Effect."[23] Boerhaave's further explication is somewhat obscure, but I would interpret it in the following manner: Cause and effect are actually inseparably one, but, when we think of things successively rather than simultaneously, what we first conceive we call the cause, and what comes later, the effect. Translated into modern terms, this seems to mean: a correlation taking the form "if *X*, then *Y*," or "*X* implies *Y*," expresses the causal nexus. This we can interpret as a timeless conjunction. But if temporal sequence does enter in, then *X*, the antecedent, we can call *cause*, and *Y*, the consequent, the *effect*. Such would seem to be the core of his remarks.

It was customary in the eighteenth century to distinguish a remote or *predisposing* cause, which included the attendant circumstances, and also an inciting or accessory or *occasional* cause, which was the trigger mechanism. Both together produced the disease,[24] and the terms are self-explanatory. In our notation we could say: There is a set of circumstances (or *predisposing* factors) such that, if *X* (the *inciting* cause) supervenes, then *Y*, the effect, will follow. That is to say, "if *X*, then *Y*" applies not under all possible conditions but only in certain particular states which we call *predisposing*. The framework must be in place, the groundwork laid, before the occasional cause can work. In medical terms, the predisposing cause might be a particular type of bodily constitution or state of the humors or solids. The accessory, inciting, or occasional cause could be, for example, something taken into the body, "whether Air, Food, Drink, Medicine, or Poison . . . in a Form either visible or invisible,"[25] or some action done by or to the body. The two causes together yield a disease, and the disease *is* the conjunction of these two causes. And obviously, the nature of the disease will vary according to the predisposition and the inciting factors.

The scientist seeks to establish correlations whereby he constructs some causal relationship in the above sense. In our symbolism, either X or Y or both may be sensory observations. Or, one may be a hypothetical construct, arrived at by "just" reasoning. Or, there may be interpolated, between X and Y, various new discoveries or concepts, X', X'', X''' and so on, which may be either observed data or hypothetical constructs. The term "necessity" which Boerhaave interjects, indicates a naïve view of causality and was written before Hume. But the notion of correlation, which to present-day thought is essential, was to Boerhaave a significant factor, often lost sight of but nevertheless real.

Scientific method concerns itself, among other things, with checking and testing alleged correlations. How can we tell if asserted connections are "really" true? This, the basic critique of inductive reasoning, received considerable attention from Francis Bacon. His various tables of agreement, of difference, and of degrees or comparison are a primitive form of logical critique rendered more clear, two centuries later, by John Stuart Mill. It is quite probable that Boerhaave had read Francis Bacon or at least was familiar with his teachings.

One goal of science is to determine whether a relationship is truly causal or merely fortuitous or casual. The essence of investigative method was quite familiar to Boerhaave, from the physical sciences, and his formulation is well worth quoting in full: To demonstrate the cause of any phenomenon, we must demonstrate the "Co-existence or immediate Connection between the Cause and its Effect . . . if the Cause of the Being A, resides in that of B; then the Entity B being supposed, that of A will also necessarily subsist; and as B is either increased, diminished, or totally abolished, so consequently will that of A: and the reverse, that is, as the Effect A appears either increased, diminished, or removed, the Cause B must be so likewise."[26] Here we have a superb expression of what later became known as the "canon of concomitant variation." Unless phenomena vary concomitantly, we should not assert a causal connection between them. The essence of causal relationship is this principle of functional dependence. Without such interdependence, a relationship is casual, not causal.

Boerhaave showed further insights into scientific method, drawn largely, perhaps, from Bacon. He mentioned, for example, that we should collect all the observations and experiments that we possibly can "and dispose them under their proper Heads" and then he gave various examples of this method,[27] reminiscent of Bacon's analysis of heat in the *Novum Organum*. And he understood the proper relationship of fact and theory, that is, "just Reasoning." Facts, he declared, must be "*thoroughly considered* in every respect; from comparing those Reasonings with Nature or Experience, and with each other; and from diligently remarking which of them appear agreeable or disagreeable to Truth; that from the whole we may be enabled to draw just Conclusions in regard to present and future Events; which Conclusions may then be relied on with Certainty."[28] Theories, he said in essence, must be tested against experience in order to be reliable, and he also hinted that theories should be tested against each other to establish consistency. And future experience will provide a test for present truth.

These were praiseworthy doctrines, well enunciated but honored more in the breach than in the observance. Boerhaave did not practice what he preached. He knew what to do according to sound canons of scientific thought. There is no doubt Boerhaave thought himself well grounded in science, considered himself progressive, empirical, logical, rigorous, and believed he had successfully avoided the speculative errors of his predecessors. Yet from our own present-day standpoint all this was not so. Can we explain the discrepancy? Francis Bacon, perhaps, gave the answer. He predicted with uncanny accuracy the errors that Boerhaave was to commit.

Bacon was the great English expounder of the new scientific impetus. Resolutely condemning the speculative philosophy of the Middle Ages, he trumpeted the call to the new science: observation, experiment, and inductive reasoning give power over nature. On the other hand, he warned that premature speculation, concern with primary causes, and untoward emphasis on deduction lead to intellectual disaster. Well versed in scholastic lore, he described and indicted faulty thought-habits which vitiated any scientific approach. And behold, Boerhaave, a century later, committed all the faults that Bacon warned against!

Bacon's *Novum Organum* is amazingly applicable to Boerhaave's writings. To paraphrase Bacon, the besetting sin is, having started with concrete observations, to "fly" to general rules. Having achieved generalization by a flight of fancy, we use these principles to *deduce* certain consequences. Then, assuming the original generalizations to be true, we feel that the deductions are inescapably true. Bacon declared, "There are and can be only two ways of searching into and discovering truth. The one flies from the senses and particulars to the most general axioms, and from these principles, *the truth of which it takes for settled and immovable* [italics mine] proceeds to judgment and the discovery of middle axioms. And this is the way now in fashion."[29]

"Middle axioms," when arrived at deductively depend for their truth on some more general principle and not directly on facts. When such intermediate axioms arise by deduction, they may commit the fallacy of begging the question. Boerhaave was very often guilty of this fault; a few instances are quite instructive. For example, he maintained that the vascular channels in the body comprised many different classes, of descending magnitude, each carrying progressively more minute and more subtle particles and humors. Now the technique of anatomical vascular injections was well developed at this time. Boerhaave pointed to some injection preparations of the brain in which the tissue, after injection, was macerated in warm water until all the brain tissue was washed away. This left only the injected vessels, which, fixed in alcohol, resembled a delicate red fleece, whose smallest branches were only one one-hundredth the diameter of a hair. Then, reasoned Boerhaave, how much finer and more delicate were the smaller vessels not filled by injection and washed away by maceration.[30] How did he know? Well, having assumed the existence of very fine vessels, much smaller than capillaries, he used the fact that they were not injected to prove how extremely fine they really were. The "existence" of such channels in the brain he derived as a "middle axiom," from his general principle that vessels much smaller than blood capillaries did exist. This, unproved, furnished the principle from which other conclusions were derived.

Boerhaave's neurological concepts furnish another example. He described two cases of apoplexy. One patient fell and died instantly. Another was similarly apoplectic but recovered in a few days. In the first, declared Boerhaave,[31] the extravasated blood immediately compressed the cerebellum with fatal outcome; in the second, the blood compressed only the cerebrum. There were no autopsies, so how did he know? These statements were conclusions, intermediate axioms, from the general principle that the cerebellum innervated the vital functions. Hence, if a man died at once, it *must have been* his cerebellum that was injured.

Bacon went on to say, in his nineteenth aphorism, that the proper method "derives axioms from the senses and particulars, rising by a gradual and unbroken ascent, so that it arrives at the most general axioms last of all. This is the true way, but untried." Bacon would have wished Boerhaave to demonstrate by definite and positive experimental data that the brain had vessels smaller than the blood capillaries. Such experiments would allow a generalization regarding the brain. Then other organs could be investigated, concretely and specifically, each separate investigation allowing a limited conclusion. After a sufficient number of such limited generalizations, it would then be proper to make a more sweeping generalization. Bacon's method was to collect data and to draw a limited inference, then to collect more data and to draw further limited inferences, and eventually, with such solid building blocks, to erect durable and reliable general principles. Boerhaave did nothing of the sort. From Bacon's view, he jumped to broad conclusions, which he treated as first principles, and from them deduced subordinate principles —proximal explanations, etiologic factors, rules of therapy —which he presented as established truths. They followed deductively from first principles, but the latter were themselves shaky and invalid.

Bacon, in his sixty-ninth aphorism, discussed some factors leading to error. In the first place, the actual "impressions of sense" may deceive us; that is, the very observations themselves may be faulty. Second, from our "impressions of sense" our "notions" (concepts and inferences) may be "indefinite and confused." Our thinking may be so muddled that, even starting with correct data, we make wrong associations and connections. Third, we err if we proceed only by "simple enumeration," that is, merely noting positive examples of a phenomenon. Instead we ought to "employ exclusions and solutions (or separations) of nature." This I interpret as demanding the recognition of negative and contrary instance, to compare with the positive examples. And finally, as the fourth source of error Bacon emphasized his previous contention, that the "curse of all science" is the method whereby "the most general principles are first established and then intermediate axioms are *tried and proved by them*" (italics mine).

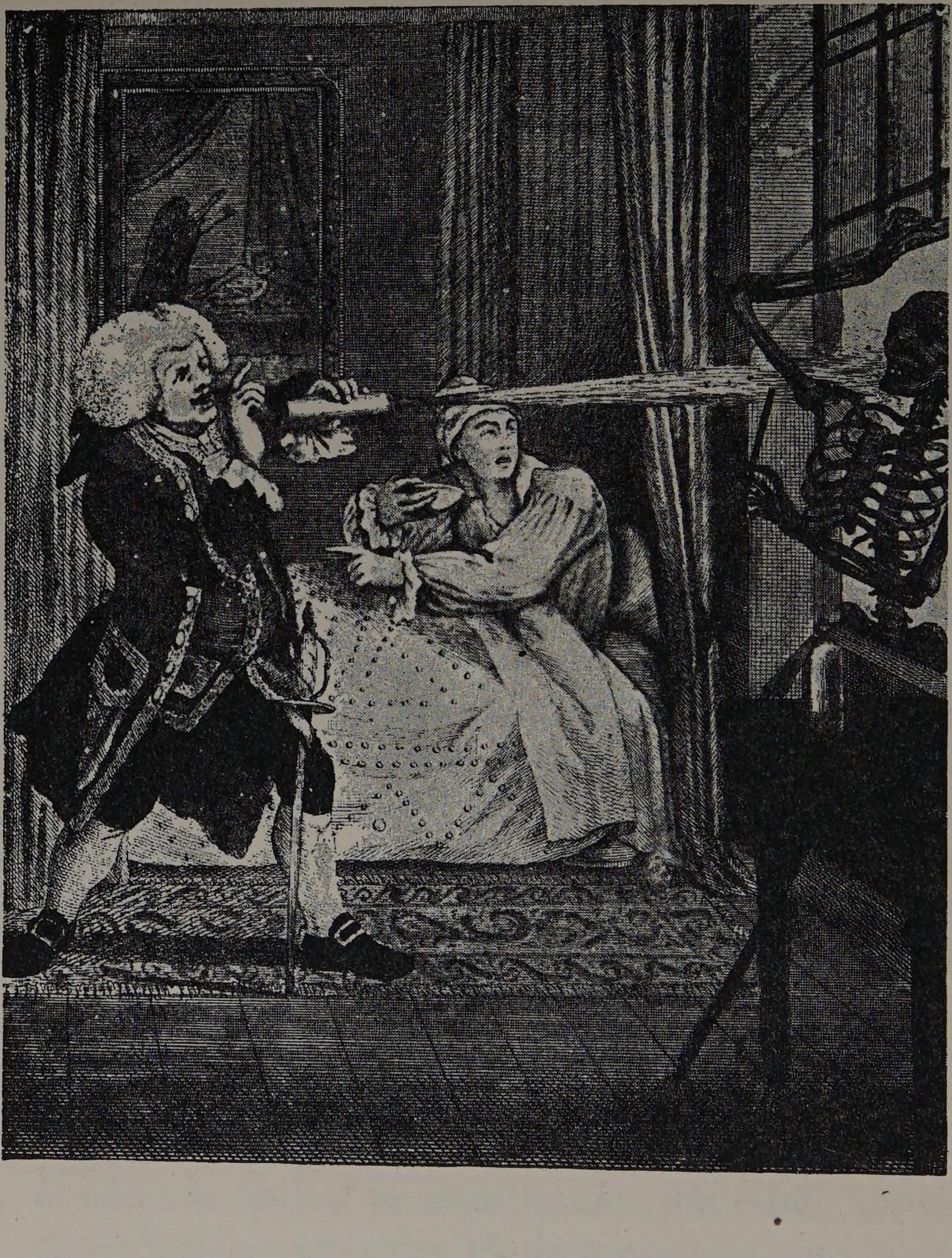
The proper way, according to Bacon, has a very modern ring. "The true method of experience . . . first lights the candle, and then by means of the candle shows the way; commencing as it does with experience duly ordered . . . and from it educing axioms, *and from established axioms again new experience*."[32] Here we have the principle of verification broadly hinted. Some proposition deduced from a more general principle will carry certain implications which are subject to verification—that is, they must lead to new predictable experience. If such verification comes to pass, the original assertion is strengthened and the general principle from which the assertion was deduced is to that extent reinforced.

One further declaration by Bacon deserves mention, that in any investigation we must go beyond "the theory itself; the inquiry must be enlarged, so as to become more general."[33] This I interpret as an effort to avoid tautology. To explain phenomena we must bring in new data. We cannot use a "virtus dormativa" to explain soporific action. We cannot take the original phenomena and, to produce an explanation, merely rephrase our description. We must connect the old with something genuinely new. Boerhaave all too often offered explanatory principles of "powers" and "forces" which were only tautologies, adding nothing to the original phenomena and quite valueless in scientific procedure.

Francis Bacon offered an excellent guide to scientific method, largely similar to present-day teachings: Begin with observed facts; proceed carefully with progressive step-wise inductions; generalize cautiously; use adequate controls; make deductions from general principles, but be sure such conclusions allow experiential verification; correlate the subject under study with other and new sensory data; avoid tautological vacuities. All these exhortations, valid today, occur in Bacon's writings, although they may nestle obscurely between terse, clipped phrases on the one hand and sheer verbosity on the other. They do not comprise a complete method for scientific advance and in many ways are very inadequate, but nevertheless they are guides which, had they been followed, would have served Boerhaave well.

The good scientist, in modern critiques, is neither intellectually muscle-bound nor mentally pedestrian. He must indeed soar and speculate and theorize way beyond the evidence, but he must always seek verification and must be keenly aware of discordant facts when they occur. He must be very sensitive and adjust his concepts to harmonize with new experience. The obtuse scientist is like a paranoid schizophrenic whose concepts are rigid and who twists facts and data to conform to his theories. He is insensitive to discrepancies. What he cannot warp to his own theories he ignores or denies.

This is the tragedy of Boerhaave. From the standpoint of scientific method he made innumerable errors, but so do medical scientists today. He drew sweeping conclusions on slender evidence, but so do eminent physicians today. But whereas now the good scientist adapts his theories to new discoveries, Boerhaave appraised evidence in light of his theories. He shared certain paranoid qualities and thus closed the door to discovery of general truths. Particular correlations he made aplenty, but his generalizations therefrom were faulty. They were incorrect, which is a common and not insuperable failing; but they were, in addition, not adaptable to change, which is a disastrous flaw. Boerhaave was surprisingly modern in many ways. His faults, the faint paranoid tinge, were not so much a matter of logic as of human personality and the spirit of the times —sheer inability to "see," to appreciate discrepancies or contradictions—one of the fatal Idols of the Cave.



V

Of Fevers

All men are mortal: all will die, of some particular disease; if it isn't one, it will be another. In the past 250 years the diseases responsible for death have changed their relative importance. Today it is cancer and arteriosclerosis that arouse the most dread. In the early eighteenth century these conditions, far less important relatively, were heavily overshadowed by the great killer, fever. Many were the kinds of fever, varied the manifestations. Fever could attack the same individual many times; in one or another form it caused about eight out of every ten deaths. A great deal was known about it, yet there were great disputes regarding its nature and treatment. And, from our modern standpoint, there was great ignorance concerning all the essential features.

When we study eighteenth-century concepts of fever, we must avoid all condescension. To preserve a proper humility, we need only remember our present-day confusion regarding arteriosclerosis and cancer. How many kinds of tumors are there? Just how, in essence, does one kind differ from another? How to classify them? What are the causes of tumors? What is it that makes a tumor malignant? We simply do not know enough. Despite the tremendous mass of relevant partial truth that we possess, our ignorance is colossal. Perhaps two hundred years from now authors more learned than we will praise our stumbling efforts yet wonder how we could overlook obvious discrepancies, fail to see certain obvious relationships.

Quite similar was the eighteenth-century struggle against fever. Then, as now, the doctors could not sit idly by; they had to treat the patients who suffered from one or another febrile illness. At worst, doctors could slavishly follow traditional remedies; at best they could discover new and more effective treatments. In either case, as intellectual men, they developed theories to justify their practice and then derived new modes of practice from their theories. Doctors classified and analyzed and prescribed as best they could. They proceeded largely by trial and error; they frequently invented wild hypotheses as explanations; they committed all manner of logical fallacies and indulged in a great deal of *post hoc* reasoning, but they also made many keen observations. In short, when we consider the differences in available techniques, the eighteenth-century doctors behaved not dissimilarly, *mutatis mutandis,* from the way we do today when we struggle against the unknown.

There were many different approaches and much confusion. Nomenclature varied and so did primary interests. Some authors studied principally books and classical authorities; others studied patients rather than books. Some concerned themselves primarily with clinical data, others with theoretical and logical subtleties. Some observed accurately and reasoned soundly, others were slipshod and fanciful. In this chapter we can make only a small and arbitrary selection from the numerous eighteenth-century physicians who wrote on fever.

We must start with Boerhaave and his disciple, Van Swieten, whose system we have already considered at length. Boerhaave struggled to find a definition of fever. He employed the Baconian method, collecting a great many instances of fever and then trying to identify the common factor, the characteristic and distinguishing features, which ran through all the individual examples. Then he eliminated those symptoms which were not present in every febrile disease, and finally he salvaged only three distinguishing factors common to all fevers. He said, "In every fever, arising from internal causes, there is always a shivering, a quick pulse, and heat, varying in degree at different times of the fever."[1] He went on to say that while this triad—shivering, heat, and rapid pulse—occurred in every fever, only the rapid pulse was present continuously from beginning to end. Hence this one feature, the rapid pulse, could be considered the pathognomonic sign of fever, its very definition; by that alone could a physician judge a fever to be present.[2]

This is a very remarkable definition—that a fever consists in a rapid pulse. It seems quite irreconcilable with common sense. Nevertheless, by some careful distinctions Van Swieten explained away all the apparent discrepancies.

For example, there was the distinction that an acute fever proceeds "swiftly and with danger."[3] Now obviously, if a man should exercise vigorously or undergo emotional stress, his pulse would increase. This, Van Swieten admitted, was a "true fever,"[4] which, however, vanished immediately when the man rested for a brief while. Hence, since the fever terminated very quickly and *without danger,* "it cannot in any wise be properly called an acute fever."[5] This was indeed an important distinction. A "true fever," that is, a rapid pulse, might arise from a variety of causes, but an "acute fever" must have some degree of attendant disability. Very slight causes, such as exercise, passions, or diet, could give "true fever" in this sense. Van Swieten saw no absurdity in so denominating these bodily changes, since the same causes when more intense could yield a fever not only true but also dangerous.

These states, it might be urged, did not give both shivering and increased heat. Boerhaave had blunted this objection by distinguishing fevers of internal from those of external causes. The latter, as, for example, sudden and severe anger, did not produce a shivering but only a "heat." Emotions, therefore, would yield a fever, but he named the cause "external," thus distinguishing it from an infectious fever such as smallpox, which had an "internal" cause.

Now, a rapid pulse indicated an increased action of the heart. If to this we added increased resistance of the capillary vessels, we then would have the essence of fever, and all symptoms could be explained by these two principles. Thus, the cold spell of a fever—the initial chill that ushered in a true, acute fever of internal cause—derived from the contraction of the blood vessels. The blood stagnated, producing an irritation which, through the intervention of the cerebellum, increased the flow of nervous juice or animal spirits into the heart. At the same time, "the other muscles of the body shake and tremble from a too weak and unequal influx of blood and spirits into them."[6] This increased action of the heart and the increased motion of the blood produced heat which diminished and finally abolished the cold stage of the fever. The heat, in fever, stemmed from the increased resistance of the vessels, plus the increased motion of the heart, both acting together and producing "a more violent attrition of the fluid parts against each other and against the sides of their containing vessels."[7] The resistance might come from contraction of the vessels or, more often, from increased viscosity or "spissitude" of the humors.

Fever was thus basically a very simple condition, but its causes might be numerous and complex. Boerhaave reduced the causes to certain general categories. First were the *ingesta,* things taken into the body which were of acrimonious nature. These included such substances as food, drink, medicines, and poisons, any of which could irritate, obstruct, or putrefy. This is all a ponderous way of saying that some fevers result from substances which enter the body from outside. A second category was the *retenta,* the retention in the body of those things that ought to be expelled. Suppression of the menses, of the lochia in parturient women, of stool, urine, and perspiration, could cause febrile disturbances. Third were the *gesta,* or actions of the body carried to excess, such as too violent exercise, which excited fever. Fourth came the *applicata,* or the external application to the body of such things as were acrid, corrosive, or inflaming. This, the correlative of the *ingesta,* was essentially a tautology, saying only that inflaming substances, applied externally, could cause an inflammation. And lastly, as the fifth "cause" of fevers, were all the things which "produce great alterations in the humors and their motions: of which there are many, both external and internal,"[8] a congeries which included suppuration, gangrene, cancer, intense study, excessive venery—a truly catholic list.

Vagueness and tautology marked all the discussions of fever. Boerhaave found certain criteria to distinguish the febrile from the non-febrile state. Within the febrile group he did not clearly separate the "symptomatic" fevers which accompanied some localized inflammation from primary fevers without localizing signs. But he did, of course, recognize a certain obvious anatomic localization, wherein a fever drew its name from the site affected, such as quinsy or peripneumonia or nephritis.

Classification of fevers was very imperfect, and there was no orderly or systematic analysis. The concepts of acute and chronic distinguished the temporal span, but deemed more important was the degree of continuity a fever might exhibit. *Intermittent* fevers, for example, showed successive bouts or paroxysms, with complete and total remission in between. The paroxysms might occur at various intervals, giving rise to distinctions such as quotidians, tertians, quartans, or double tertians. These were quite easily recognized, and, after introduction of the Peruvian bark, often responded quite well to treatment. Far more complex were the *continued* fevers, which showed no complete remissions during the course of the illness but in which the febrile symptoms might wax and wane in irregular fashion. If the illness lasted less than twenty-four hours, it was an "ephemera"; if more than that, it was a "simple continued fever"; if severe, it might be designated "putrid." If it exhibited very intense heat, Boerhaave called it an "ardent fever." In his Table of Contents the febrile types included only *continued, putrid, ardent,* and *intermitting.* After discussing these, he proceeded to those febrile ailments showing specific anatomical localization.

Treatment was essentially symptomatic. The general goal, the restoration of health, required correction or removal of the irritating acrids; the cause of fever should be eliminated—it might be "subdued and resolved," or it might be assimilated with the healthy humors, or it might be expelled by sensible evacuations, through the perspiration, vomitus, saliva, urine, or feces. But since the cause (in our sense of an infectious agent) was not identified, this goal was only a pious wish. If the patient recovered, it was assumed that the "morbid matter" had been eliminated. If he died, it had not been eliminated.

The actual treatments, supposed to expel or correct the hypothetical morbid matter, showed some interesting concepts. Suitable attention to the six "non-naturals"—air, food and drink, motion and rest, passions of the mind, retenta and dejecta, sleep and vigilance—added up to a fairly sound hygienic regime. Abundant cooling fluids and light, easily digested foods, rest and quiet, emotional and mental calm, all these are fairly satisfactory remedies by modern standards. Promotion of elimination was perhaps excessive but probably in most cases did no serious harm.[9]

In all fevers, even of widely differing types, the general plan was the same: besides correcting the "acrid irritation" and the "urgent symptoms," the "powers of the body" had to be maintained and the "lentor" resolved or expelled. The latter two goals might require stimulants if the body was too weak, or bloodletting—phlebotomy—if there was a "plethora" or the "febrile motions" had to be diminished. Clysters, purges, and blisters also served this function. Compared with many other eighteenth-century writers, however, Boerhaave and Van Swieten exhibited a very pleasing moderation in regard to phlebotomy.

They trusted greatly to the healing powers of nature, realizing with Sydenham that physicians often tended to be overzealous in treatment and could make much more progress if they advanced with less haste. Van Swieten stated specifically that there can be no general rule for the cure of fevers.[10] What is useful in one case, he declared, may be harmful in another, and it is wrong to treat every case by a fixed regimen. Indiscriminate bleeding or purging was as bad as indiscriminate stimulations. Moderation, attention to general pathologic principles, and careful watching of the patient's condition were the basic concepts. All this called for judgment, since each case was to be individually considered.

Boerhaave and Van Swieten followed the classical authors. The commentaries abound in quotations from Hippocrates and Galen, and to a lesser degree from Celsus, Aretaeus, Aetius, and occasionally from others. Classical references far outnumber those to sixteenth-, seventeenth-, and eighteenth-century authors. As we read the voluminous commentaries, we find a great effort to harmonize the new and the old, to avoid any sharp break with tradition. There was no attempt to achieve accurate discriminations. The very lack of precision, the absence of strict diagnostic categories, emphasized the individual approach to each sick patient. Treatment of symptoms and prognosis were the important aspects of medicine, even as they were for Hippocrates and Galen.

John Huxham (1694–1768), who studied for three years under Boerhaave, continued his master's tradition. Huxham returned to England to become a leading practitioner and influential author. His well-known text, *Essay on Fevers,* first published in 1739, went through many editions.[11] A sound thinker and clear writer, Huxham expressed himself more logically, perhaps, than did his teacher. He adopted Boerhaave's anatomical principles and physiological doctrines quite uncritically but then applied these principles to febrile diseases in quite logical fashion.

He distinguished two principal types, with certain profound differences, namely, the *inflammatory* and the *low nervous* fevers. These two forms he correlated with certain physiological and bodily states. We have already seen that when the humors were too viscous relative to the vessels, inflammation resulted, and inflammation readily led to fever. If, then, the solids (fibers or vessels) were too firm, tense, or elastic, the blood too rich, dense, or "high," then an "inflammatory" fever very easily resulted. Causes might be quite varied: exposure to cold, sudden cooling of the body after exercise, checking a perspiration too quickly, heating the blood through excess alcohol—these and similar agents, as well as various acrimonies, could induce an inflammatory febrile state. The cure followed quite logically: diminish the velocity or the quantity of the blood, lessen its acrimony, relax the solids. Bloodletting was definitely indicated, as well as the promotion of gentle excretions.

But the patient might show a quite different constitution. His fibers might be soft and lax, his blood thin and poor, and the humors of a "ropy" quality. As in the preceding type, Huxham taught, there was obstruction of vessels, but the site of obstruction was the serous or lymphatic channels rather than the sanguiferous arteries. The resulting fever would then be not inflammatory but the low nervous type. The causes included a poor, watery, unwholesome diet, wet weather, anxiety of spirits. The actual fever—the slow nervous fever—was considerably less vehement than the inflammatory type, but the prostration was greater and the condition harder to treat. Bleeding, of course, was not indicated in this state, and supportive remedies were required.

The two simple or relatively pure types might, as already mentioned, have varied causation. There was a certain non-specificity which forcibly impresses the modern reader. But other more or less comparable febrile states suggested a little more specific character, and a more definite etiology. There were the definitely contagious fevers which he declared were "common to both" of the two principal forms[12] and which, when severe, were called putrid or petechial or malignant fevers. The terms referred to clinical conditions, of great severity, which generally were deemed contagious but not always or necessarily so. Huxham, to explain them, invoked a third state of the blood, different from the two preceding, in which there was a tendency to putrefaction or corruption. Putrid humors, he thought, were strongly alkali, and the alkaline acrimony of Boerhaave was the putrescent stage for Huxham. The urine, secretions, dejecta, and vomitus stank foully, the blood might corrupt spontaneously, and the body after death putrefied very rapidly.

The putrid fevers were more akin to the slow nervous fevers, and of these two conditions Huxham was "very sensible, [that] the one may be, and very often is, blended with other."[13] But there were differences. While the slow nervous fever arose from "lentor" and "vapidity" of the juices and a relaxed state of the solids, the putrid fever had its locus and principal action in the blood rather than the other juices, arose chiefly from contagion, and had different effects on different temperaments. The method of treatment was very different, depending on the patient's habitus, the state of the disease, and the signs that were manifest.

These putrid fevers, of the continued type, seemed to be approximate entities. There were obviously many other discrete febrile states, as, for example, the entire class of intermittents—the agues—which arose from the moist atmospheres of swampy soils. Huxham tried to make these a group intermediate between the inflammatory and the low nervous types, sometimes inclining more to the one, sometimes more to the other. The quotidians, for example, were closest to the inflammatory type, the tertians less close. The distinction, as well as the degree of kinship, was quite important practically, since treatment depended on the main category to which an ailment was akin. If, for example, one of the intermittents was wrongly treated, it could transform into the nervous type, or even into a putrid fever.

Huxham's classification was rather confused. He did not stress specificity, but he did achieve certain vague clinical groupings. These he tried to correlate with other data, but his knowledge was too fragmentary for successful definition or circumscriptions. He ended with a very imperfect cross-classification in which there was great fluidity and little precision.

A considerable step forward was taken by Sir John Pringle (1707–82), one of the century's most progressive physicians, who studied medicine first at Edinburgh and later at Leiden under Boerhaave. Receiving his degree in 1730, he returned to Edinburgh, where, besides medicine, his interests included both natural and speculative philosophy. In 1742 he received a commission as physician to the military hospital in Flanders and served the army for several years in the Low Countries, and also in Great Britain in 1745. His great book, *Observations on the Diseases of the Army,*[14] contained observations, reflections, and conclusions drawn from his military experience. Pringle achieved the highest honors in both medicine and science—physician to the king and president of the Royal Society. He was not only a sound observer and clinician but also a keen experimentalist who published excellent papers on putrefaction, sepsis, and antisepsis.

As an army surgeon he had great opportunities to study febrile illnesses prevalent in camps and to correlate in specific fashion certain particular diseases with various environmental factors, such as weather, camp sites, fatigue, and crowding. Pringle did not attempt a textbook, nor did he aim at completeness. He discussed only a limited number of diseases, but these in a fairly thorough fashion.

He concerned himself with the two major disease classes already familiar. In the winter and spring there occurred principally the inflammatory fevers, which usually showed topical localization—coughs, pleurisies, peripneumonies, acute rheumatism, inflammation of brain, bowels, and the like. According to current teachings, these diseases indicated that the fibers were firm, the blood condensed, and the pores contracted. During the summer and fall the fibers were more relaxed, the fluids more rarified and disposed to putrefaction. The prevalent diseases were of the putrid type, often called bilious. The term "bilious," he explained, he used more to accord with custom than to indicate pathogenesis, for "it has never been proved that either the autumnal fevers or fluxes originally proceed from a redundant or a corrupted bile."[15] Bilious fever was an ancient term, similar to a cholera, or violent discharge of bile. For Pringle the excess bile was an effect of the disease, not a cause.

For our purposes the most interesting aspects are not his keen descriptions of symptoms or the details of his treatment but rather his general classification and the discussion of etiology. Pringle recognized many causal factors but appreciated that they did not tell the whole story. It had generally been recognized that heat and cold, moisture and dampness, could all cause fever, and Pringle discussed in practical fashion the various ways in which these factors might be relevant. He also paid special attention to the "putrefaction of the air, which of all the causes of sickness is perhaps the most fatal and the least understood."[16] He noted several different sources of noxious air: from corrupted water or marshes; the vapors and "putrid effluvia" from marshy places; from human excrements lying about in camps during hot weather, when dysentery is common; from straw rotting in the tents; and from the air in crowded places, where many men are sick with putrid distempers. Excess heat or cold or moisture, putrid states of the air, great fatigue, wet clothes, all these "only dispose men to sickness, and do not necessarily bring it on."[17] Nevertheless, knowledge of these offers a powerful tool for control, since through avoiding these factors the epidemics may be checked. The theoretical importance of heat, moisture, and so on had long been recognized in a rather academic or abstract fashion. But Pringle gave concrete directions for guarding men from these causes, and he discussed the importance of suitable privies, proper sanitary regulations adequately enforced, and purified air. He deserves great credit for his practical insight and concrete suggestions, which are of demonstrable worth.

The "remitting and intermitting fevers," the so-called bilious fevers, were quite distinct from the "regular" intermittents (or agues), which, he said, were rarely met in camps. Many of the remittent patients became "yellow as in the jaundice,"[18] but an autopsy in one such patient revealed no stones in the gall bladder or ducts.

His descriptions of symptoms and the clinical course of these various remitting and bilious fevers are indeed masterly, but for our purpose his analysis of causation is especially interesting. He maintained the classical but rather confused notion of many different *sorts* of cause. The one which was "remote and external" was the heat and moisture of the air. Correlative to this was the "internal and predisposing" cause, which was the relaxation of the fibers and the greater tendency of the humors to putrefy. These two causes might sometimes be sufficient of themselves to bring about the disease. But for the most part, declared Pringle, there was required an "exciting or occasional cause." This, he thought, arose from an "error" in the "non-naturals," such as the familiar fatigue, intemperance, checking perspiration, lying on wet ground, and so on, which, to use our modern terminology, acted as a trigger mechanism. We might have hoped that, for the exciting cause, he would have demanded some specific contagion in the modern sense, but he did not. Nevertheless he was by no means satisfied with his formulations, for he added that, although we have the remote, the predisposing, and the occasional causes, we do not have the *"causa proxima* or immediate cause"—that is, we do not know just how the "vitiated humors act on the vital principle." The pathogenesis, he recognized, was not clear enough, and he was groping, unsuccessfully. Boerhaave would have closed the gap with ingenious and airy speculations, but not Pringle. Not enough was known, and rather than form speculative hypotheses he thought it preferable "to wait till further discoveries be made in the animal machine."[19] Rare discretion and self-control!

Among the remitting pestilential fevers he devoted considerable space to dysentery or the bloody flux, whose contagious nature he strongly maintained. Besides fine clinical and epidemiological descriptions, Pringle added a series of autopsy findings in fatal cases. His analysis of causation is revealing. He implicated spread by "putrid exhalation" which, received into the blood, acted as a ferment and disposed the whole mass of blood to putrefaction. But he perceived that, to make this hypothesis logical, he must assume that the "vitiated" part of the "tainted" blood "must be thrown upon the intestines for excretion," and he suggested some concrete evidence to indicate that this actually took place. He originally considered the immediate cause to be this putrid ferment, but, having learned of contagion by *animalcula,* he suggested suspending all hypotheses until the matter was further pursued.[20]

Of the putrid fevers Pringle gave a superb description of the malignant jail or hospital fever, clearly recognizable today as typhus. This seemed to be a more or less discrete entity, but he did not consider it strictly specific, since he felt that any putrid disease might lead to this malignant fever. Recognizing that there were many malignant or pestilential fevers, he explained the variety "according to the kind and quantity of the virulent *miasma* received into the blood."[21] How the ferment acted was a matter of conjecture, but he suggested that it could corrupt the whole mass of blood and affect the brain and other organs in inflammation. Localization in the brain kept up the fever, and cure obtained only when this obstructing matter was resolved or suppurated. This inference was not mere imagination but drew support from autopsy observations, for in a few cases of supposed jail fever autopsy unexpectedly showed suppuration in the brain.

The malignant and pestilential fever had some affinity with the familiar so-called nervous fever. The conditions so designated, he pointed out, appeared to belong sometimes in the "inflammatory" group, sometimes in the "autumnal class" of diseases. Nevertheless, if the fever showed "petechial spots, putrid sweats, or become contagious," he would conclude that the humors had become putrid and that the fever had "changed into one of a pestilential nature, akin to that of hospitals and jails." He tried, he declared, to distinguish the entire group of malignant or pestilential fever from all other types "as far as I could do it in distempers whose symptoms are so much alike."[22]

Pringle showed a very decided advance over his predecessors. In the first place, he did not worship at the Hippocratic shrine; he oriented himself to current writers rather than to the wisdom of the ancients. He did, in large part, use the existing theoretical and conceptual frameworks—for he knew no other—but he was in no sense a slave to system. There was no systematic strait jacket to confine his thoughts. Instead, he allowed free play to his keen powers of observation and drew cautious inferences from his observed data. His inferences are very remarkable for their moderation. He was not afraid to suspend judgment, to admit ignorance, and preferred to leave data unexplained rather than to formulate some fanciful hypotheses. Pringle exemplified the true modern investigative spirit.

The advance in method, attitude, and general approach was very marked. As for content, there was also considerable advance, but not so marked as in methodology. His descriptions of disease, especially of typhus, were excellent. But more significant were his gropings. He tried to define the specificity of disease entities but did not quite succeed. He appreciated the significance of contagion and "ferments" but could not satisfy himself regarding their exact causative role. Recognizing the state of ignorance and the need for further information, he wisely refrained from undue speculation but pointed the way to future progress.

Approximately contemporary with Pringle was George Cleghorn, a naval surgeon stationed in Minorca. He wished that his predecessors at that post had furnished some observations on the prevalent diseases but, since no one had, he decided to repair the deficiency and to record whatever data might aid a thorough knowledge of local diseases and their cure. He kept a careful diary from 1743 to 1749, recording data on the weather, the customs, habits, and diseases of the inhabitants, and the various epidemic and sporadic diseases. He wanted to record his experiences, both successful and unsuccessful, to indicate to others what could be beneficial or hurtful in similar cases.

Cleghorn lacked the imagination and genius of Pringle. His concepts were far less precise, and his notions retained the vague fluidity of Boerhaave, without sharp discriminations or circumscriptions. He was a meticulous observer, described his data carefully, performed autopsies, mentioned relevant data. His book was a sound piece of work, one which any physician in southern climates would be glad to have.[23] But it is noteworthy more for the fine empirical attitude, the patient observations and attention to detail, than for any substantial advance in the understanding of disease.

The leading British physician of the eighteenth century was probably William Cullen (1710–90), whose teachings and influence gradually superseded Boerhaave's. Cullen, a brilliantly endowed Scotsman, had served as surgeon's apprentice and as ship surgeon and had engaged in private practice before he matriculated in the University of Edinburgh in 1734. His actual medical degree he received from Glasgow in 1740. A man of intense application, he mastered not only the practice of medicine but the sciences of chemistry, botany, and materia medica. He taught these subjects at Glasgow and afterward at Edinburgh. Here he became professor, first of the institutes of medicine and later of the "practice of physic." Since Edinburgh was at this time the leading school in Great Britain, Cullen can rightly be considered the foremost British clinician of his day.

Cullen felt the need to form a system analogous to Boerhaave's. Much new information had "been acquired by observation and experiment." By incorporating it all into a systematic whole, every particular subject was, he thought, improved and thereby "renderd more complete, consistent, and useful."[24] Unfortunately, a passion for system-making commits an author to hypothetical constructs that usually far transcend experience.

A very important influence was the current nosologic trend. As we shall see in Chapter VII, Cullen was himself a leader of this movement. Much as modern writers ridicule the nosologic excesses, the desire to classify served very important ends: it promoted sharp definitions and helped to differentiate one entity from another, to make men conscious of subtle differences. It demanded careful observations to achieve suitable distinctions.

Cullen carefully separated the secondary or symptomatic fevers, which accompanied topical inflammation, from those which were "essential" or primary. The latter he classified under two main types, the intermittent and the continued. The intermittent, comprising the familiar agues, the quotidian, tertian, and quartan fevers, need not detain us. More important are the continued fevers, of which he distinguished the *synocha,* which corresponded roughly to the inflammatory fever, with a strong, hard pulse, and no disturbances of the sensorium; and the *typhus,* with great prostration, weak pulse, and delirium. Cullen appreciated, however, that types, rarely pure, tended to flow one into the other. Hence he distinguished a third category, the *synochus* fever, embracing cases which were partly synochal, partly typhus.

Cullen as a systematist aimed at correcting Boerhaave's errors. The great Dutchman, we have seen, invoked a "lentor" of the humors, with resulting obstruction, as the essence of inflammation and fever. Cullen, an acute critic of other men's errors but curiously blind to his own unwarranted assumptions, realized that there was no positive evidence for this hypothetical "lentor." Influenced by Friedrich Hoffmann and the rudimentary neurophysiology of the day, he substituted a "spasm" of the arteries in explaining the pathogenesis of fevers.

According to his concepts, fevers exhibited three distinct stages, first a debility, then a state of cold, which in turn led to the hot stage. He was unsure of the mechanisms but correlated the sequence with changes in small blood vessels, from atony to irritation and spasm, and then to relaxation, mediated through heart action and nervous energy. Cullen, having criticized Boerhaave's "viscosity" and "lentor" as hypothetical, adduced evidence to implicate vascular contraction in fevers. He emphasized organic interrelations among the body tissues and directed attention away from circulating fluids toward the solids—the vessels and the nervous system.

Impressed by the epidemic character of many fevers, Cullen thought that "some matter floating in the atmosphere, and applied to the bodies of men, ought to be considered as the remote cause of fevers."[25] These effluvia were of two sorts, the *contagions* arising from human bodies suffering from a particular disease and capable of exciting the same disease in some other person; and *miasmata,* stemming from marshes and moist ground, that is, from sources other than human.

Cullen did not believe there was any great variety of separate contagions. There were the exanthemata, which were specific, but the chief problem regarded the continued fevers. Concerning these he was unsure. He thought there probably was only one principal common source of all such contagions, constantly arising from living bodies and somehow acquiring a peculiar virulence if long retained in one place. The normal human effluvia became virulent if kept from diffusing freely. The difference between various febrile states he thought due not to distinct contagions but to different "circumstances of season, climate, etc., concurring with the contagion and modifying its force." Maintaining considerable misgivings regarding this point, he was not at all dogmatic. He recognized that jail or hospital fever seemed somewhat different from other forms and thought that the responsible contagion was not "permanent and constantly existing," but was "occasionally generated."[26]

Earlier eighteenth-century doctrine had invoked a very wide variety of factors causing fevers, which Cullen reduced to a proper perspective. The chief role of contagions or miasmata he stressed, but he could not dismiss all other causes. He paid particular attention to cold and to temperature changes, which, indeed, were quite often correlated with fevers. And then he mentioned, briefly, some of the other factors, as strong emotions or intemperance, which might have some secondary or "concurrent" causal relevance.

Treatment followed logically on his theory but did not mark any great advance over his predecessors. To our modern view no one theory has any advantage over another unless it makes some practical, concrete difference in therapy. But Cullen, although he made things orderly and neat, could not add any significant practical therapeutic contribution. To treat fevers, he taught, we should remove spasm from the extreme arteries, and this we could accomplish by familiar remedies: by allaying irritation, by sedation and rest, by avoidance of animal food, by judicious applications of cold, and by bloodletting, practiced with discrimination. Purges and blisters were also valuable.

"We know nothing of the nature of contagion," declared Cullen, "that can lead us to any measures for removing or correcting it. We know only its effects as a sedative power inducing debility, or as a ferment inducing a tendency to putrefaction in the fluids."[27] Therapy, therefore, could not directly attack the contagious material but aimed at moderating the force of reaction and correcting the debility, as well as combating putrefaction with rational hygienic measures. Interestingly enough, these are exactly the same therapeutic principles that remained in vogue well into the present century, until the discovery of antibiotics. Phlebotomy and blistering lost their popularity, in the nineteenth century, but inability directly to combat the contagion remained until very modern times.

The basic differences between Cullen and Boerhaave do not seem so very profound after two hundred years. When we compare the two men, we find that their therapeutic tools were not dissimilar: they both stressed the need for judgment and discrimination; and, confronted with an actual case, they would probably have behaved rather similarly. Cullen, however, had a far more empirical and open mind and seems far more advanced in scientific medicine, an advance more in attitude, perhaps, than in concrete accomplishment. His clear writing, critical acumen, and analytic powers are impressive even today. But his scientific attitude needed new factual data, new techniques and tools, to become truly fruitful. Without these he could only stand at the threshold of significant scientific progress; he could not, himself, enter.

Although Cullen was misguided and uncritical in many respects, he nevertheless was a scientist, with the scientist's respect for truth, patient observation, and a "just reasoning." But his pupil John Brown (1735–88), brilliant and vastly influential, allowed his passions to dominate his intellect. The Preface to his *Elements*[28] tells the story: He started to suffer from gout while still a young man, and at first adhered to the current belief that this disease depended "upon plethora and excessive vigor," treated by abstemious living, vegetarian diet, and avoidance of wine. Brown claims that, although he kept to this regimen, there was a whole year during which he suffered severe attacks almost continuously. Almost by inspiration there came to him some new concepts, "that debility was the cause of his disorder; and that the remedy was to be sought in strengthening measures." He sought to banish his gout by strengthening himself. He avoided debilitating foods, ate only "animal food," avoided fish and vegetables, and for stimulation used wine and opium. From independent sources it is known that he used liquor and opium very vigorously indeed. In this way he believed he was able to repel the disease. Evidence is not at all clear that he cured himself of gout, but it is well established that he became heavily addicted to alcohol and opium. And his writings and doctrines furnished an excellent way to justify his habits.

Brown disregarded the mechanistic schools which explained phenomena by physical concepts, even if merely hypothetical. Viscid fluids, acrid particles, spasmodically contracted vessels, were of course not so much observed data as they were hypotheses. But at least, even though not actually observed or measured, they were theoretically observable and ponderable. It so happened that with further study they proved to be erroneous, but they were nevertheless potential, if not actual, physical entities.

Brown, however, went back to the vitalists for his intellectual kinship. He followed those threads in seventeenth- and eighteenth-century thinking that led eventually to the excesses of Schelling, Hegel, and *Naturphilosophie.* He started off with the concept of *excitability* as the basis of his teachings. Excitability distinguished the living from the dead. When you had it, you were alive; if you had it in due proportions, you were well and healthy; if in too great or too small amount, you were ill; and if not at all, you were dead. Excitability, whose seat was in the nervous system, was affected by stimuli, which might be external or internal and constituted the exciting powers, or *excitement.* The relationships between the excitability and the excitement "explained" all bodily states. Too little stimulation (deficient excitement) was bad, constituting *direct* debility from defect of stimulus. On the other hand, excessive excitement would overstimulate. This was also bad, since it too might cause debility by exhausting the excitability. Such a state was *indirect* debility.[29] The analogy to a fire has been suggested.[30] If there was not enough air (insufficient excitement), the fire would smolder or go out. Under a forced draft (too much excitement) the fire would burn excessively, become exhausted, and go out. The analogy must not be pushed too far, for Brown's notion of excitability is a morass in which the intellect, if it approaches too closely, flounders and sinks. But some relevance of the metaphor is apparent.

"It is the excitement alone, through its varying degrees, that produces either health, disease, or recovery. It alone governs both universal and local diseases; neither of which ever arise from faults of the solids or fluids, but always from increased or diminished excitement."[31] There were two kinds of disease. Those arising from excessive excitement he called *sthenic;* those from deficient excitement, *asthenic.* General diseases always had a preceding predisposition or diathesis. Of the two disease types it was supremely important to determine which one was operative. Through excessive or injudicious treatment we might change one diathesis or predisposition into the other; hence injudicious remedies might cause disease of the opposite type. Thus, gout was an asthenic disease, for which the proper remedy was stimulation. But stimuli carried too far changed an asthenic ailment into a sthenic one through the mechanism of indirect debility. Improper treatment of gout might produce a cough, a catarrh, or an inflammatory sore throat.

There is a certain magnificent and reckless fluidity to the whole doctrine, which can explain anything. The whole practice of medicine was all quite simple, for "when a physician comes to the bedside of a patient, he has only three things to settle in his mind. First, whether the disease be general or local; secondly, if general, whether it be sthenic or asthenic; thirdly, what is its degree." Once these questions were answered the treatment was simple: "All that remains for him to do, is to form his indication or general view of the plan of cure, and carry that into execution by the administration of proper remedies."[32] The remedies, of course, were to stimulate the asthenic conditions, to allay excitement for the sthenic. It was all quite cut and dried, and a physician could practice by formula; he would not need to study the individual patient. It is no wonder that Brown's doctrines became popular.

The distinction between sthenic and asthenic diseases was similar to that between inflammatory and debilitating fevers, but it was expanded to embrace not only the febrile conditions but all diseases whatsoever. The concept of indirect debility, however, introduced an impenetrable confusion. If excess stimuli could yield a clinical picture indistinguishable from deficient stimuli, there was no sure way of telling which cause was operative, and yet the wrong remedy would make the disease worse. The only way to tell, it would seem, was after the fact: if treatment made the patient worse, the excitement had been changed in the wrong sense—it had been raised when it should have been lowered, or *vice versa*. If, however, the patient recovered, the treatment had presumably been correct.

Brown placed fevers—true fevers—among the asthenic diseases. Here were the intermittent fevers and typhus, "pestilential typhus, the jail, putrid, or petechial fever, and the Plague."[33] On the other hand, among the sthenic diseases he placed those febrile states with local inflammation, such as peripneumony, erysipelas, cynanche tonsillaris, as well as smallpox and measles and simple continued fevers.[34]

Contagion played only a small role, since the causes rested with stimulation or debility, not with contagion. An example of Brunonian logic is the following: "The small pox and measles are cured by the same means as peripneumony or any other sthenic disease; and, excepting the contagious matter, arise from the same stimulant hurtful powers; they must, therefore, with the same exception, be the same. The only difference is, that they are accompanied with a contagious matter, and the other sthenic diseases are not. The amount of which is altogether unimportant."[35] This airy dismissal of the role of contagion indicates how far removed was Brown from the truly scientific thinkers of the period.

Brown, we must emphasize, was a legitimate physician and in no sense a quack or charlatan. His doctrine obviously would appeal to the poorly trained, the feebly discriminating. His system marked an undertow in the tide of eighteenth-century progress.

A transition from John Brown in England to Benjamin Rush (1745–1813) in America comes very naturally. Both were students of Cullen, both elaborated rather similar doctrines, both felt themselves misunderstood and persecuted. Rush was a very complex and controversial character. There is no doubt that intellectually he was very gifted and frequently very clear-thinking, that he made many good and patient observations, that in many ways he was quite progressive. But his personality was so rigid and self-righteous, his outlook and adaptability so limited, that they raise the question of a paranoid trend.

Rush, owing quite obvious debts to Boerhaave, Cullen, and Brown, became entrapped in the same morass that enveloped Brown—debility, excitement, and excitability. Preceding all fever there was, he claimed, a state of general debility, which arose from either too much or too little stimuli, either of which, by lowering the excitement, could produce debility. Once this happened, there followed a sudden accumulation of "excitability" followed by a "reaction" of the blood vessels. The essence of fever, its proximate cause, was an "irregular action or convulsion" of the blood vessels. When this was present, and only then, did we have fever.

Rush held that there was only one fever, since there was only one proximate cause. Fever, he declared, was a single entity, just as fire is a single entity, "whether it be produced by friction, percussion, electricity, fermentation, or by a piece of wood or coal in a state of inflammation." He went on to say, "All ordinary fever being seated in the blood vessels, it follows, of course, that all those local affections we call pleurisy, angina, phrenitis, internal dropsy of the brain, pulmonary consumption, and inflammation of the liver, stomach, bowels, and limbs, are symptoms only of an original and primary disease in the sanguiferous system."[36]

But he did finally have to accept multiplicity somewhere. At least there were many different factors—heat or cold, marsh and human miasmata, contagions, poisons, passions, and the like—which induced a fever. Any of these, as remote causes, might lead to fever through their stimulating power. But the disease remained the same whether the remote factor were heat, contagions, or miasmata. Much of this supposed variety depended on where the morbid excitement took place. Sometimes it was diffused equally through the entire vascular system, other times it involved only part of the body. Thus, the heart and lungs might be affected by great morbid excitement while the arteries of the wrist were normal. Or one side of the body might be affected more than the other, or vessels in the viscera might be highly excited while those in the extremities were feeble.

Fever, then, was an irregular action, or convulsion, of the arteries. The term "irregular" is significant. Boerhaave, we have seen, had defined fever as a state with a rapid pulse but could not distinguish adequately between real fevers and mere tachycardia from exercise. Rush solved the problem by changing the words. After exercise there was indeed an excessive action of the arteries, but this was not fever because it was *regular*. The heightened action in fevers, on the other hand, was irregular. Unfortunately, he did not provide adequate criteria to distinguish regular from irregular, other than calling the febrile pulse "jerking."

Rush spun many fine verbal cobwebs elaborating all this, and denying his dependence on Cullen or Brown, but examination of these cobwebs would be of little value here. His discussions mark a step backward from Cullen.

Rush was, in general, an acute observer and a conscientious physician. Until 1793 his actual treatments of fever, in the concrete and practical sense, probably did not deviate significantly from his contemporaries and predecessors. But in this year occurred the severe yellow fever epidemic in Philadelphia, the greatly dreaded and highly malignant putrid fever. The current forms of therapy were not too successful. Rush vividly described his experiences and told how he changed his methods to the drastic procedures now associated with his name. In one of the very dramatic passages he tells how, despairing over previous therapeutic failure, he recollected some old manuscripts dealing with the epidemic of 1741 that he had once read. Consulting these papers again, he was greatly impressed by certain passages which, like a sudden religious illumination, showed him the true way to proper therapy: purge, purge, and purge some more.[37] Very soon a new illumination supervened. This one indicated that extensive bleeding should complement the purging. Why? To remove the excess "stimulus" from the system. He declared, just as if it were a religious exaltation, "Never before did I experience such sublime joy as I now felt in contemplating the success of my remedies. It repaid me for all the toils and studies of my life."[38] And never did religious bigot pursue his course more fanatically than did Rush pursue his bleedings and purgings. He was entirely convinced that he had found the true way. Others, however, were not convinced. In fact, many were aghast and horrified. For the present we can leave Rush, weltering in blood, serenely convinced that he was acting for the welfare of his patients.

The great yellow fever epidemic of 1793, which Benjamin Rush described in Philadelphia, had probably entered the United States from the West Indies, where the disease was more or less endemic. The year 1793 marked an unusually severe outbreak. Physicians who as army surgeons or civilians practiced in the West Indies during the 1790's had unrivaled opportunity to study fevers. Colin Chisholm left a most valuable record of his experiences. He distinguished the "malignant pestilential fever" from the "yellow remitting fever." The latter, he thought, was an endemic disease whose causes were marsh miasmata, exhalations from stagnant pools, heat, excessive exercise, night air, dew, abuse of spirituous liquors, and the like.[39] On the other hand, malignant pestilential fever arose from contagion alone. Cutting off all source of contagious matter prevented the spread, but exposure to contagious particles promoted the spread. With Chisholm the distinction between contagion and miasma became especially acute because of the practical problems involved.

Chisholm, following the contemporary chemist S. L. Mitchell, tried to explain disease through the chemical theories of the day. Mitchell believed that definite chemical compounds caused pestilential fevers and that the responsible agents were combinations of gases arising from putrescent animal and vegetable substances. The atmosphere, he thought, was composed of two gases, and hence the "various deviations from the proportions of oxygen and azote which fix the standard of health . . . give rise to various characters of disease." To the extent to which the "azote" predominated, so would the "malignity of the morbid actions" increase. It was the "gaseous oxyd of azote, or nitrogene" which, according to Mitchell, was the basis of that poison we call contagion.[40]

This theory seemed to dispel the mystery surrounding contagious disease. Chisholm strongly denied that all fevers had a uniform cause. The malignant pestilential type arose from contagion, but the yellow remitting fever did not. The latter arose from marsh effluvia, which, therefore, must differ from the gaseous oxyd of azote that caused contagion.[41] This seems logical enough if we grant the premises. Chisholm felt, with Hunter, that the remittent fevers arose from exhalations from marshy ground, and therefore the "examination of these exhalations is the first step in the inquiry." The investigation would certainly be "delicate" but must not be despaired of.[42] Simple moisture and "inflammable air" could readily be eliminated. Chisholm suggested that whereas "azote and oxygine," chemically combined, produced the contagious and pestilential fevers, it was "hydrogene" alone or combined with carbonic gas that excited the intermittent or remittent fevers. He expressly stated, however, that he offered this as a mere suggestion, from combined "speculative reasoning" and "practical observation." He hoped that the suggestion might lead eventually to "useful inquiry."[43]

That his theories were all wrong does not diminish his significance in the history of fevers. He tried to merge contemporary scientific data into medical thinking in a fashion that Benjamin Rush, professor of chemistry though he was, never tried to do. "Azote" and "hydrogene," as hypothetical causes of fever, may seem pretty silly, but they show a distinct advance over "excitement," for example. Chisholm was on the path of progress, but it is the nature of progress to be slow and to make many false turnings.

Another prominent physician who spent many crucial years in the West Indies was Robert Jackson (1750–1827). His great book, *Sketch of the History and Cure of Febrile Diseases,* first appeared in 1817, but, since the observations were made in the West Indies before 1798, it is not inappropriate to include this work within our selected period. Jackson, who had only imperfect formal medical education, recognized two sorts of causes for febrile diseases, miasmata and contagions. Miasmata, the "exhalations from the surface of the earth," produced the ordinary endemic. But "on some occasions an extra or adventitious quality is joined with it which, in a manner we do not comprehend, . . . constitutes a form of disease which obtains the name of epidemic."[44] An epidemic was thus only an endemic fever peculiarly modified in some unknown manner. Other fevers were due to contagious materials, "emanation from diseased animal bodies."

Whatever the type of fever, the causative agent did not act instantaneously. There was always a definite incubation period. In some cases the "cause" might remain dormant for several months before getting "excited into action." The nature of this action was obscure. Jackson believed the "cause" circulated in the blood, until somehow it involved "a certain series of capillary vessels which constitutes a function. At this organic point, the action of health is subverted and the action of disease commences."[45] He argued strongly that disease was not merely a diminished "energy of actions" or a debility or a "sedative" quality. On the contrary, he said, disease "is irritative of new and unnatural actions throughout the whole system, or in particular parts of it."[46]

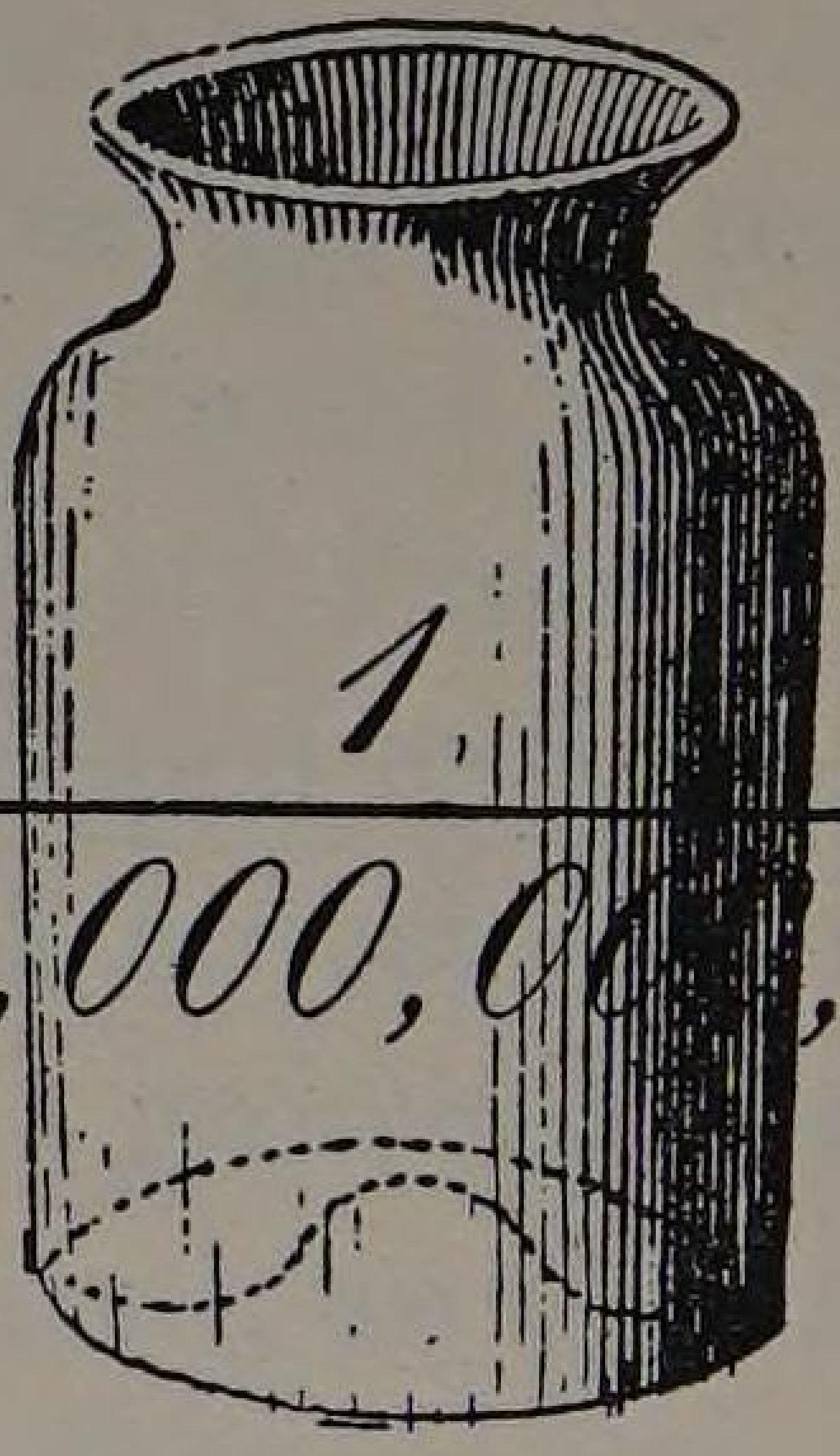
Jackson considered endemic fever to be essentially one disease, which took a variety of forms, "inflammatory, putrid, nervous, bilious, simple, complicated, mild, malignant, etc., from the operations of a cause which is distinctly and demonstratively one."[47] What, then was responsible for these variations? If the inciting agent was the same, the diversity must find explanation in the patient himself. Jackson attributed the variations in disease-form to variations in constitution or temperament of the patient. These differences, in turn, he correlated with particular appearances and reactions of the blood and with the bodily habitus. The way the blood coagulated, he thought, the relative amounts and the appearance of the crassamentum, the fibrin, and the serum, reflected the temperament and constitution of the patient. On this basis he disinguished six different temperaments, bearing the formidable names, sanguine, phlegmatic, serous, gangrenous, phlegmatic-liquescent, and serous-liquescent.

Jackson observed certain properties of blood and tried to associate these with hypothetical bodily temperaments. He thought that there was a dependable correlation, but unfortunately the alleged connections proved unreliable. The appearance of the crassamentum was an inadequte foundation for a durable or fruitful hypothesis, but the effort itself was a most encouraging sign.

The great yellow fever epidemics he studied at considerable length. The disease, he thought, was not spread by contact but was due to some general infection of the air. The crowding of people in poorly ventilated quarters, with other "artificial cause," were factors which would "multiply or aggregate the noxious material, whatever it may be." These factors facilitated "the operation of this noxious material" to give at first sight an appearance of contagious propagation. He suggested that as a "consequence of general irritations" there was some propagating process generating "a material which, under certain circumstances of aptitude, possesses the power of self propagation or continued multiplication." Furthermore, there was an "aptitude" to contract the disease, strongly present in newly transferred soldiers, which diminished with residence. But there were numerous factors, including change from one locale to another, crowding in ships or barracks, dissipation, and the like, which increased the aptitude. Since Jackson held to the unity of all endemic fevers, he could scarcely form any doctrine of specific immunity. But glimmerings of epidemiology and immunity he certainly possessed.[48]

When we read Jackson's book, we appreciate his earnest strivings. Clearly, febrile disease took different forms. Clearly, some factor or factors must be responsible for these differences. But what? From time to time we see a flash of insight which helped to illumine the confusion.

Chisholm and Jackson, however they differed from each other, both exemplified a growing new trend in medicine. To a physician on the scene, an epidemic can sharpen the acuity and at the same time reduce and concentrate the field of attention. Instead of a vague general "system" which was all-inclusive and could explain everything, we find close attention to a specific field of endeavor. Instead of vagueness, there is more desire for precision. Instead of logical completeness, we find concern for insistent practical details. Rush, to be sure, was also in the thick of the 1793 epidemic. He also faced brutal practical issues. But with all his great intellectual gifts, his precocity, his sound academic training and self-righteous integrity, his mind was not flexible. In the study of febrile disease he faced toward the past rather than toward the future. Jackson and Chisholm were also wrong in many ways. Their inferences and conclusions were largely incorrect. But they heralded the new spirit of the nineteenth century, the spirit of eager youthful inquiry, of flexibility, of a new empiricism, which, repeatedly proved wrong, nevertheless persevered undaunted. There was repudiation, not of the past, but of the restraint which the past had imposed. There was, in short, the rise of a new perspective in which speculative and experimental tendencies could, after suitable discipline had been achieved, disregard the fetters of authority and tradition and seek truth for itself. Medical science, as the eighteenth century drew to its close, was thus experiencing a true renaissance.



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VI

Similia Similibus

Profound scholarship that lacked common sense. Penetrating intellect that could not see the obvious. Great logical acumen that ignored facts. These attributes characterized Samuel Hahnemann, a gifted individual who, when beaten down by a hostile environment, created his own environment where he was supreme. He invented a new and vastly influential system of medicine which he persuaded many to accept. Critics had no trouble finding inconsistencies and absurdities but, nevertheless, homeopathy, his brain child, grew and prospered. Even during Hahnemann's lifetime Oliver Wendell Holmes, with great wit, quite demolished the entire doctrine.[1] But somehow the devastating criticisms failed to destroy. In the next few years, the demolition occurred again and again,[2] yet homeopathy still retained its vitality.

Homeopathic doctrine and literature developed primarily during the nineteenth century and therefore might seem out of place among the present studies of eighteenth-century medicine. But actually homeopathy represents eighteenth-century thinking; the basic tenets were laid down during this period, as a revolt against contemporary excesses. The doctrine employed eighteenth-century logic, developed around eighteenth-century concepts of physiology and pathology, and remained an eighteenth-century holdover which flowered in the nineteenth century.

Homeopathy, as a doctrine, stems directly from the personal life of Samuel Hahnemann. Not too much is known about his early days. Born in 1755 in Meissen, where his father was an impoverished porcelain-painter, he even as a child displayed scholarly instincts. It is said that since his father opposed too much education, a private tutor taught him without fees because of his brilliant promise. However this may be, we know that in 1775 at the age of twenty he left home for the city of Leipzig, intending to study medicine. In his pocket were twenty thalers, a gift from his father, and with this small patrimony he faced the world alone. In Leipzig he attended medical lectures, supporting himself meanwhile by German translations from English and private tutoring. After two years, for reasons that are not clear, he left Leipzig for Vienna, where he continued his studies for a few months until his money gave out. Then at the end of 1777 he took a position as librarian to a Baron von Bruckenthal, cataloguing not only a fine library but also an extensive coin collection. Here he remained for almost a year and a half, spending most of his time in study. As one biographer describes his great linguistic ability, he was a "master" of Greek, Latin, English, Italian, Syriac, Hebrew, Arabic, Spanish, and German, and in addition had "some smattering" of Chaldaic.[3] Ancient literature and occult sciences, as well as numismatics, occupied his time. The value of this preparation for a medical career is, of course, debatable.

In the spring of 1779 he went to the University of Erlangen, where, after attending sufficient lectures, he received his medical degree in August of the same year. His dissertation was entitled "Etiology and Therapeutics of Spasmodic Affections."

At last a doctor, Hahnemann began what I wish to call his "period of frustration." Dissatisfied with medicine and practicing it only spasmodically, he wandered from town to town. A list of places and dates will emphasize this nomadic existence. It appears that in the fall of 1779 he spent some months traveling in Hungary and then located in Hettstadt for nine months. Then he went to Dessau and, in 1781, to Gommern, where he married. Here he was the parish doctor but was scarcely a success. He left Gommern in 1784, going first to Dresden, then to the nearby village of Lockowitz. By this time he had given up private practice and earned his living chiefly by translating. For one year he was in charge of the local hospital but apparently the appointment was not renewed. By 1789 he went back briefly to Leipzig, desperately poor, later leaving the city for the nearby village of Stolleritz, where he lived very meagerly indeed, meanwhile writing and translating.

In 1792 fortune smiled briefly upon him. In an earlier paper he had discussed humane and gentle treatment of the insane. When the Hanoverian minister of police, Von Klockenbring, became patently insane, his wife, who had read that essay, arranged for Hahnemnn to treat her husband. The Duke of Gotha devoted a wing of his castle as an asylum, and here Hahnemann directed the treatment. Apparently Von Klockenbring was the only patient. Later Hahnemann (in 1796) published a report of this case which even today is a fascinating account.[4] Under Hahnemann's ministrations the patient recovered in March, 1793, and the physician, although well rewarded, was again out of a job. Hahnemann was strictly contemporary with Pinel in introducing modern humane psychiatric approaches. But such posthumous recognition buttered no contemporary turnips.

For two more years he wandered from one small village to another. The year 1795 saw him settled in the town of Königslutter, where he remained for four years. Forced to move for reasons which will appear shortly, he wandered first to Altona, then, in 1800, to Hamburg. Two years later he was again on the move, poorer than ever, never staying long in one place. His family was literally hungry, and he himself, it is said, helped his wife wash clothes, using raw potatoes instead of the soap they could not afford. He had again given up practice but continued his writings.

Brighter days were ahead, however. In 1805 he settled in Torgau, resuming practice apparently with considerable success; he remained there for six years. No longer was he compelled to translate foreign books in order to eke out a living. His own system of medicine was by this time fairly well formed and he attracted considerable attention. At this point, when he faced success after twenty-six bitter years of wandering, we leave him for the moment, to trace the development of his doctrine.

Hahnemann, a prodigious linguist, earned his living in large part by translation. In the first dozen or so years after getting his degree he rendered into German such diverse volumes as *Water and Warm Baths, History of the Lives of Abelard and Heloise, A Treatise on the Materia Medica, Art of Manufacturing Vinegar, Advice to the Female Sex in Pregnancy, Art of Making Wine,* as well as many others. At the same time he engaged in extensive chemical studies, publishing such original papers as "On Arsenical Poisoning," "On the Influence of Certain Gases in the Fermentation of Wine," "Discovery of a New Constituent in Plumbago," "Exact Mode of Preparing the Soluble Mercury" (complete bibliography in Bradford).[5] Unfortunately most of his early work is entirely unavailable to the modern student but must be judged by title only. We can, however, appreciate that Hahnemann was a thorough scholar. One biographer noted that the study on arsenic poisoning quotes 861 passages from 389 different authors. How much original or valuable material this chemistry paper contains, is a different question which his biographers pass over rather quickly and which in any case is not relevant to the present study.

During this wandering period Hahnemann did not have a very active medical practice. That subtle assurance that comes from seeing many sick people he simply did not have. No amount of scholarly reading can substitute for the clinical judgment that comes only from extensive experience. His translations and his chemical studies were not adequate substitutes. In part, therefore, the numerous extravagances in homeopathy arose from Hahnemann's lack of experience with patients.

In his early years Hahnemann showed sound balance and good judgment. He was highly critical of contemporary medical practices. As early as 1784 he urged, as modes of treatment, exercise and nourishing diet and pure wholesome air, to "give strength to the muscles . . . attenuate the humors so that they can readily penetrate the remotest capillary vessels."[6] Most surprising because they are all eminently readable today are the chatty, informative essays on medical subjects originally published in 1792 and 1795 under the title *The Friend of Health.*[7] For the control of contagious disease he advocated abundant fresh air, cleanliness, and ordinary sanitation. In prisons, to avoid the dreaded jail fever, for example, he urged cleansing each cell and heating the air to a temperature of 120 degrees Réaumur by a portable stove. In epidemics he advocated rigorous supervision of old-clothes dealers and rag-gatherers as well as other hygienic principles which, although not original, were thoroughly sound.

Hahnemann quite early became disillusioned about current medical practice. The contemporary therapeutics seemed totally irrational and, moreover, positively detrimental to any genuine progress. Treatments were given and patients frequently got well, but the relationship of any single therapeutic agent to the final recovery was obscure. Diets and depletions, baths, clysters, fomentations, venesections, and inunctions were mingled with complicated prescriptions, all in helter-skelter order. Prescription-writing seemed to him the height of folly. One medicine was called the base, another the adjuvant, another the corrective, and others were the director or the excipient. Why were particular medicines given? What was their rationale? What was their effect? How did you know? Questions such as these received no satisfactory answer. Empiricism and tradition dictated the therapy, while the actions of pure drugs were virtually unknown. The irrational combinations of medications completely prevented the study of individual drug action. Hahnemann, in brief, decried the absence of any experimental or scientific pharmacology. Moreover, he was highly critical of the pharmacists. If a doctor sent a complicated prescription to ten different pharmacists, he would get back ten preparations differing in taste, appearance, and smell, to say nothing of medicinal properties. As we shall see, Hahnemann's answer was to use not complicated mixtures but single remedies.

Hahnemann's bitter feelings toward the pharmacists were reciprocated. The enmity of the apothecaries helped drive him from one town to another and undoubtedly played a significant part in developing his system of infinitesimal doses. Parenthetically we should note that Hahnemann's father-in-law was an apothecary. Whether this personal relationship contributed fuel to the fire is a speculation impossible to settle but worth keeping in mind.

The criticisms that Hahnemann raised against current therapeutic procedures were quite valid. That his own therapeutic system proved a tissue of absurdities in no way detracts from his objective censure of others. Hahnemann opposed not only the apothecaries but also the practicing physicians, especially those of fashion. His essays in *The Friend of Health* include a delightful caricature of the fashionable doctor who, with thirty house-visits to pay, starts out in his carriage:

> Doors fly open at his approach, three steps bring him to the patient's side. He feels his pulse, asks him a couple of questions, and without reply calls for pen, ink, and paper: and after deep reflection for two seconds in his chair he suddenly dashes off the complex prescription, politely hands it to the patient . . . makes his bow and disappears, in order to be with another patient six seconds afterwards, on whom also he bestows his two minutes of advice.[8]

Allowing for exaggeration, there was much truth in this sketch. No wonder that Hahnemann was a failure as a medical practitioner. He had a scholar's instincts, which rebelled at contemporary practice.

The turning point in Hahnemann's life came in 1790, when he translated into German a new edition of Cullen's *Materia Medica*. Speaking of cinchona, "the bark," Cullen mentioned its effectiveness in intermittent fevers, due to the "strengthening power it exerts on the stomach." Hahnemann, in a footnote, rejected this view, and gave the first fragmentary expression to his peculiar doctrine. He put his new indeas to an experimental test:

> For the sake of experiment I took for several days four drachms of good cinchona bark twice a day; my feet, finger-tips, etc. first grew cold. I became exhausted and sleepy; then my heart began to palpitate, my pulse became hard and rapid; I had intolerable anxiety, trembling (but no rigor), prostration in all my limbs; then throbbing in the head; flushing of the cheeks, thirst, and in short all the ordinary symptoms of intermittent fever appeared one after another, but without actual febrile rigor. . . . This paroxysm lasted two to three hours each time and returned when I repeated the dose, otherwise not. On leaving off the drug I was soon quite well.[9]

Several points here deserve comment. In 1790 there was, as we have already seen, no sharp criterion of fever. What constituted a fever? When did the patient have a fever? No one answer was universally acceptable. As we have noted previously, the condition of the pulse was a widely used standard. Other accessory symptoms clustered around this one feature. Contemporary teachings found no absurdity in Hahnemann's diagnosis of *fever* on the basis of the enumerated symptoms, however absurd the doctrine seems today. If we wish to pass judgment on outmoded theory, we should judge it in its contemporary context.

Second, there was considerable opinion that fever was a unitary disease state. A miasma produced a fever. Cinchona produced a fever. For Hahnemann and most of his contemporaries the two states were therefore quite equivalent. This was sound reasoning for the eighteenth century.

Third, Hahnemann ran a controlled experiment in pharmacology. He started with a single "pure" substance, cinchona. (Component alkaloids were quite unknown then.) He used a healthy normal subject—himself. Ingestion of the drug caused the same symptoms repeatedly, and the symptoms passed off when the drug was not taken. He served thus as his own control, alternating periods of experiment and periods of rest, and finding a correlation between symptoms and the experimental procedure and between absence of symptoms and periods of rest. How many experiments he performed we do not know, but from the standpoint of formal scientific method the procedure is quite excellent and one that is frequently repeated in modern medical literature.

The experiment would have been much stronger had there been other subjects. A series of one case is to us a weak foundation for sweeping conclusions. But, to that one case conviction can be profound and absolute. We can imagine him quite convinced: "This is a fact. This happened to me. This is genuine experience. However you define truth, this is true. You cannot tell me that it did not happen. You might as well claim that the sun is not shining when *obviously* it is." Such an attitude has many counterparts. Every religious mystic *knows* the validity of his experience and merely smiles when you say that it didn't happen. A similar conviction possessed Hahnemann.

In 1790, then, Hahnemann enunciated the basic fact on which homeopathy was later founded: a medicinal substance could produce a disease. Cinchona, which cured fever, could itself produce a fever. Or at least it produced certain symptoms which Hahnemann *called* fever. The following year he took the next step, going from one instance to a widespread generalization. In 1791, in his translation of Munro's *Medical and Pharmaceutical Chemistry,* he tucked away more about cinchona. The bark, he said, "overpowers and suppresses the intermittent fever chiefly by exciting a fever of short duration of its own. . . . All other substances capable of exciting counter-irritation and artificial fever, given shortly before the paroxysm, check intermittent fever quite as specifically, but they cannot be relied on with such certainty."[10] He mentioned such diverse substances as ipecac, pepper, brandy, and burned coffee with lemon juice as exerting this antagonistic action.

If cinchona alone produced this peculiar effect—inducing the same disease that it cures—we would have merely an isolated phenomenon, interesting but not of great scientific significance. If, however, we were able to generalize, if we could show that cinchona was simply an example of a more general rule, if it were one instance among many, then we could make true scientific progress. If we could go from a single drug to a whole series, if we could show that essentially the same action inhered in the entire series, then we would have laid the groundwork for the systematic development of real science.

In 1796 Hahnemann published a most important work, entitled "Essay on a New Principle for Ascertaining the Curative Powers of Drugs, with a Few Glances at Those Hitherto Employed."[11] If we seek knowledge on drug actions, chemistry can tell us many things, but very cogently Hahnemann pointed out that properties observed "in the chemical phial" are not the same as properties in the body. He fully appreciated that *in vitro* results are not the same as *in vivo*. To find out the action of drugs, experiments on animals are an obvious procedure but by no means adequate, for the body of an animal is not the same as the body of a human. Drugs fatal to humans may produce no effect on animals. Investigators might, of course, test drugs on many animals (or animal species) at once, and observe carefully for dangerous effects. But such experiments are at best "rude and awkward" and not in any way "decisive." Here he was on very solid ground, much more solid, perhaps, than that which supports some current scientific studies. A test tube is not a living body, and what occurs in pig or monkey need have no necessary relevance to what occurs in humans.

To lay the basis for rational therapeutics Hahnemann recommended experimentation directly on humans—healthy humans. Diseased persons, he said, are not suitable subjects for experiment, since diseases can induce various complicating factors, so that even the "greatest practical genius" cannot determine what part is played by the remedy, what part by the disease. Furthermore, medicines to be tested must be used pure, unmixed. The great difficulty is to recognize and identify just what action the drug is exerting. Since objective measurements were at this time not adequately developed, the only reliable indicator was introspective verbal report. This is the basic postulate underlying homeopathic materia medica: small and subtle physiological effects are *all* reflected in consciousness, so that, merely by observing his own state of consciousness, the observer can find and describe all the physiological actions produced by the drug. There is the further implication that what does not appear in consciousness is quite irrelevant, pharmacologically speaking.

Another colossal assumption lies implicit in Hahnemanns' work, namely, that after ingestion of a drug whatever appears in consciousness, whatever feelings and sensations the subject experiences, are all causally related to the drug. It is a glorious "post hoc ergo propter hoc." Today these assumptions appear absurd, but in the eighteenth century they were not unreasonable. They were certainly plausible, considering the state of biological science, and no more ridiculous than many other eighteenth-century assumptions.

This early paper explicitly set forth two basic homeopathic principles, which Hahnemann called "axioms." That is, he did not attempt any proof but assumed them as self-evident without proof. The first stated that every powerful medicinal substance induces in the body a peculiar kind of disease. This, clearly, reflected his own original experiment with cinchona. That cinchona was a powerful medicinal substance was obvious. That it produced a peculiar sort of "disease" in healthy people was, to Hahnemann himself, equally obvious. He simply generalized from one drug to all drugs. The second axiom declared that in the disease we wish to cure we should employ "that medicine which is able to produce another very similar artificial disease."[12] That is, cinchona cured certain fevers, but cinchona also produced in normal people (such as Hahnemann himself) a reaction interpreted as a fever. By simple reasoning we conclude that what produces a fever cures a fever. This is the doctrine of *similia similibus*—one disease is cured by another similar to it.

The two principles declare, then, first, that medicines produce disease in healthy people; and second, that a naturally occurring disease is cured by a medicinally induced disease. It follows that, for any given illness, we must choose a remedy which, in health, will induce a very similar condition. And, conversely, any drug which, in health, produces certain "symptoms" will cure those exact symptoms in a naturally occurring disease.

Does a drug in a normal person produce a tingling sensation? Then in appropriate dosage it will cure tingling sensations that arise in disease. Hahnemann said, for example, that coffee in large doses produces headaches. Therefore, in moderate doses it cures headaches, or, at least, certain types of headaches. In large doses coffee favors peristaltic motions of the bowels, so in small doses it cures chronic diarrhea. These conclusions follow from the axioms mentioned above. According to Hahnemann's theory, these things *ought* to happen. He had no difficulty persuading himself that they *did* happen.

Since the axioms and the deductions therefrom arose from his original experiment with cinchona, all the theoretical elaborations were simply superstructure, to preserve and enshrine that original experience. Hahnemann, of course, adduced further supportive "evidence" which consisted principally of wishful thinking rather than scientific data. It is scarcely profitable to analyze the details further, since Hahnemann's notions of evidence and proof were quite different from ours today.

For most people, the term "homeopathy" connotes very minute or infinitesimal doses. This was a gradual development. As late as 1798 Hahnemann recommended the use of camphor in large doses. He began with fifteen to sixteen grains per day and went as high as forty grains, with marked improvement.[13] Within a year or two, however, he was recommending very small quantities. The change may have arisen from his long-standing feud with the apothecaries.

Hahnemann had violently opposed the complicated prescriptions of his day. The apothecaries held a rigid monopoly on dispensing drugs and, of course, the more complicated the prescription, the more profitable was their trade. Hahnemann prescribed one drug at a time, as pure as possible. And, since he was a chemist and an authoritative writer on pharmacology and materia medica, he dispensed his own drugs, in accordance with his homeopathic law of similars. During his stay at Königslutter he collided head-on with the pharmacists, who, he claimed, were incited by the jealous physicians of the town. The apothecaries, incited or not, brought action against him for interfering with their traditional privileges. Hahnemann defended himself vigorously, claiming that the guild privileges extended only to the *compounding* of medicines. The right to sell or give *uncompounded* drugs, he claimed, was not involved. It is rather a nice point, but he lost his plea and was prohibited from dispensing his own simple medicines.[14] Consequently, he was forced to relinquish practice in Königslutter, where he had lived from 1795 to 1799. If he could not prescribe medicines the way he wanted, then he could no longer practice. And since he would not come to terms with the apothecaries, he had to move.

His last year there, however, was most important, for there occurred an epidemic of scarlet fever during which he made many significant observations.[15] Previously in treating scarlet fever he had used minuscule doses: Having prepared a tincture of opium (crude opium, 1 part; weak alcohol, 20 parts), he moistened with this tincture a strip of filter paper one-half to one inch long and laid it on the pit of the stomach. Then for internal use he took 1 drop of the tincture, diluted it with 500 drops of dilute alcohol, took 1 drop of this which he again diluted with 500 drops more. One drop of this final mixture, he calculated, contained 1/5,000,000th of a grain of opium. Treatment consisted of 1 to 2 drops.[16]

During the epidemic in Königslutter he made a great discovery. The mother of a large family developed a severe sore throat, and shortly afterward the ten-year-old daughter became ill. Among other symptoms the child had pain in the abdomen, itching, restlessness, headache, pain in the throat, stiffness of the limbs. She also had "an air of the most dejected pusillanimity. . . . her look was dull and yet staring, the eyelids inordinately wide open." Hahnemann at this point recalled, that belladonna could produce similar symptoms, that "it has, according to my observations, a tendency to excite even in healthy persons, great dejected pusillanimity, dull staring (stupid) look, with inordinately opened eyelids . . . pressive headache, constrictive pains in the abdomen."[17] Therefore, consonant with his doctrine of similars, he gave the child belladonna, giving only the 1/432,000th part of a grain. Twenty hours later she was remarkably improved and, shortly after, completely well. Hahnemann reasoned that whatever could so quickly check a disease at its onset would probably be the best preventive. So he gave the remaining five children in the family the same drug and they all remained perfectly well.

Hahnemann recalled a previous family he had treated, where, of four children, the eldest, who was suffering from a joint disease, had been receiving belladonna. This child did not contract scarlet fever, although her three siblings, not having received the medicine, did become ill. This observation confirmed his ideas: belladonna was a preventive as well as a cure for scarlet fever. This prophylactic use of belladonna he considered a great discovery.

And as for the very small doses? His reasoning here is very interesting. He declared that in illness the body is enormously more sensitive to drugs than in health. He admitted that it seemed incredible that a sick person should exhibit violent reaction to a very minute dose, "and yet this is undeniably the case."[18] As analogy he pointed out that for a healthy person it would take an enormous quantity of soup to make the stomach vomit; yet in an acute fever the smell alone may induce vomiting. The millionth part of a drop, coming into contact with the mucous membranes of the nose, will produce violent reaction.

It is wrong to ask, What effect *can* 1/100,000th of a grain of belladonna have? The proper question is, What effect *does* 1/100,000th of a grain of belladonna have? Hahnemann pointed to the results of small doses: the patients recovered from scarlet fever or were protected from it. That *is* the effect, "incredible as it may seem."

Thus, by the year 1801 the two major principles in homeopathy, the curative action of similars and the effectiveness of minute doses, were established. Hahnemann's later writings expanded, elaborated, buttressed, and embroidered these basic doctrines, or dogmas. At this point we may consider again Hahnemann's personal life.

When he lost his struggle to practice as he wished in

Königslutter, his affairs were very black indeed. He settled briefly in Altona for a few months, then moved to Hamburg. Here he tried to capitalize on the Königslutter scarlet fever epidemic by selling the secret remedy he had discovered. He tried to secure paid subscriptions and promised to reveal the secret to the subscribers. Actually he got very few subscribers but very much abuse, so much that he hastily published his results describing the belladonna treatment.

In this period also belongs another unsavory incident, regarding the supposed discovery of a new medicinal substance. This he named "Alkali Pneum" and offered it for sale through an agent. Critics soon disclosed that the alleged new remedy was only common borax. Defending himself from calumny, Hahnemann declared the incident an honest error in chemistry and that, on becoming aware of it, he had refunded all the money he had received. It is indeed highly unlikely that Hahnemann ever stooped to wilful, conscious deceit. But he was probably so desperate for a little financial reward, so hungry for a little measure of renown, that he was incapable of good judgment.

About this time his voluminous writings assumed a new style. Gone is the (moderately) light touch that occasionally characterized some earlier essays. Now his words showed bitterness and venom. His criticisms became sharp and personal. There was a noticeable loss of objectivity. Formerly his inferences may have been faulty, but his approach was urbane and, within limits, objective. By 1801 to his questionable logic he added abusive and scornful language. These changes correlated well with his severely depressed fortunes.

After Hamburg he wandered farther, never staying long in one place until, in 1805, he settled in Torgau, where he remained for six years before moving to Leipzig. It was during this period that he at last found substantial recognition. In 1810, while still in Torgau, he published his *Organon of Rational Healing,* his most important work, which went through five editions in his lifetime and was translated into many languages, including French, Hungarian, Spanish, Italian, Swedish, Russian, and English. In 1811 he began publication of the *Materia Medica Pura,* that is, materia medica along homeopathic principles. The first volume appeared in 1811, the sixth in 1821. The *Organon* elaborated the basic principles already discussed; the *Materia Medica Pura* recorded the symptoms which various medicinal substances produced on the healthy human body. These tests, or "provings," assumed considerable importance in Hahnemann's system, and to them we shall return shortly.

When Hahnemann moved to Leipzig in 1811, his doctrines had jelled. He had a system of medicine all his own, entirely opposed to current medical practice. The regular physicians, whom he called "allopaths," repudiated him. The pharmacists opposed him. Nevertheless, he was making headway, attracting not only considerable sympathetic interest but, even more important, a growing number of patients. In Leipzig he wished to attain some status in the university. He wanted the privilege of delivering lectures, a privilege he could obtain by defending a dissertation and paying a suitable fee. Instead of discussing his own controversial doctrine, he wrote "A Medical Historical Dissertation on the Helleborism of the Ancients," a work of fearsome erudition and minute scholarship, quite divorced from any practical problems in medical practice. Nevertheless, he was granted the privilege of conducting lectures.

The period of frustration gradually merged into a period of recognition. There was no sharp dividing line. All through the 1790's his doctrines were taking dim shape.

The more they were ridiculed or, what is worse, ignored, the more he retreated within himself, elaborating and refining and spinning new logical threads. About the turn of the century, just when he was, materially speaking, the worst off, his teachings were becoming solid and rigid. This apparently gave him new strength, and his new system emerged, with sufficient vitality to break through the frustrations and to survive.

Hahnemann not only forged an original doctrine but created an environment in which this doctrine could flourish. At the university he advertised lectures wherein he wanted to show "by the evidence of sight that the truth of this doctrine stands firmly upon an irrefutable basis in its whole extent, and that the homeopathic method of healing, new as it is, is the only acceptable, the most consistent, the simplest, the surest and the most beneficent of all earthly ways of healing human disease."[19] Surely, adversity had not induced humility. A six months' course was sufficient to teach the principles of homeopathy. Hahnemann gave two courses of lectures a year from 1812 until 1821, when he left Leipzig. He turned to younger students and physicians, who were more open-minded or, at least, not so firmly prejudiced as were the older practitioners. Over a period of years Hahnemann built up a vigorous band of disciples.

He was becoming an institution. A coterie of flattering students and admirers surrounded him. He held court, so to speak. Patients he saw only between nine and twelve in the morning and two and four in the afternoon. Then he, with his wife and daughters, regularly took walks in the late afternoon. Evenings he liked to spend conversing with trusted friends to whom, needless to say, he spoke oracularly. As a parent, he believed in "the old German system of training children. The children displayed not only obedience, but the most hearty love towards their parents."[28] We, however, can reasonably doubt the heartiness of this love when we read in the very next paragraph, "Although living in luxurious and elegant Leipsic, yet the daughters of Hahnemann took no part in any public amusement; they were clad in the simplest fashion, and undertook most cheerfully the humblest household services." Hahnemann was not one to encourage independence or self-expression in his children. The same student described Hahnemann's somewhat less-than-perfect marital relations: "His tall and stout wife who gave him many a bitter hour, exercised the most baneful influence upon him. . . . It was she who often caused dissension between himself and his most faithful pupils if they did not treat the doctor's wife with the deepest respect. Notwithstanding this, Hahnemann was accustomed to call this scolding Xantippe, who took pleasure in raising a storm in the house, 'the noble companion of his professional life.'"[21]

We can perhaps feel for him, scolded at home, scorned, derided, and attacked from without. We can understand how, when he did achieve success, he became very intolerant, would brook no contradiction, and viewed with suspicion anyone who differed from him. He is reputed to have said, "He who does not walk on exactly the same line with me, who diverges, if it be but the breadth of a straw, to the right or to the left, is an apostate and a traitor, and with him I will have nothing to do."[22]

The years he spent in Leipzig were probably on the whole very happy, until implacable opposition again began to hound him. Again the apothecaries sought to enjoin Hahnemann from dispensing medicines. It was Königslutter all over again, with claim and counterclaim. Hahneman maintained that he did not compound prescriptions but gave only single medicinal substances, and that this procedure did not conflict with apothecaries' prerogatives. The decision, however, went against him. There has never been any satisfactory answer to the question why Hahnemann could not have compromised, somehow, with at least one pharmacist who might then have made the appropriate dilutions under supervision. Certainly later on there were homeopathic pharmacists. It is also significant that, not very long after Hahnemann left Leipzig, homeopathic physicians were permitted the self-dispensing of medicines. It seems probable that Hahnemann's truculence and antagonism prevented any peaceful solution. He always insisted that the homeopathic physician must prepare his own medicine, but the reason is not clear. By this time he probably had a martyr complex and delighted in having enemies.

In 1821, when he was sixty-six years old, he left Leipzig for the small duchy of Anhalt-Köthen, whose duke tendered him unlimited privileges within the duchy. Köthen was a small town of about six thousand, quite different from brilliant Leipzig. Hahnemann lived in partial retirement, still seeing many patients, still writing and corresponding voluminously. Meanwhile, homeopathy grew. A homeopathic journal was founded. Homeopaths increased in number and influence. As a movement, homeopathy was entering a period of recognition. But for Hahnemann this period merged gradually into a stage of intellectual calcification. He became even more remote from reality. He saw only those things which he wanted to see. Out of his inner consciousness he would spin out extravagant verbiage that bore little relation to the everyday world of sick people. During this period he not only continued revising his *Organon* but published, in 1828, a book, *Chronic Diseases: Their Nature and Homeopathic Treatment.* This volume furnished abundant merriment to all the opponents of homeopathy, for it attributed virtually all disease to psora, the itch. However, Hahnemann was not perturbed. He continued writing, reading, and seeing patients, and dominating his small environment.

In 1830 his wife died and Hahnemann went into semiretirement, his daughters keeping house for him. Then, in 1835, occurred the most fantastic episode of his long and fantastic life. His reputation had reached the ears of a youngish Frenchwoman, Mlle Melanie d'Hervilly Gohier, who determined to meet this prodigy of learning and skill. She was the nineteenth-century version of the emancipated woman who could ride, swim, shoot, and do as she pleased. Furthermore, she was an artist, an intellectual, and a woman of independent means. One evening, dressed as a man, she arrived at Köthen. The next morning, much to the surprise of everyone at the inn, she metamorphosed into a handsome woman and visited Hahnemann as a patient. Whatever the nature of her illness, which is not important, there was a mutual attraction between this thirty-five-year-old cosmopolite and the eighty-year-old scholar. Much to the dismay of Hahnemann's daughters, the old man and the young patient promptly got married and almost immediately set off for Paris. It is said that she traveled dressed in man's clothes and that they passed as father and son rather than as husband and wife.

In Paris began a new existence. Hahnemann's wife exerted influence with the minister of public instruction, François Guizot, securing for Hahnemann permission to practice medicine. He became quite the rage, the fashionable doctor. Patients came from far and wide to consult him. His practice was enormous—and profitable. Furthermore, he led an active social life, all adroitly managed by his wife. There is little doubt that Mlle Gohier was a remarkable woman and that she made Hahnemann's last years very happy. A more pronounced contrast it would be hard to imagine than that between life with the German shrew and his final years with the Frenchwoman. He was quite thoroughly insulated from the various derogatory comments that spread among his opponents. With his wife as devoted manager, he was famous, respected, petted, and rich. Greater earthly felicity he could not ask. He continued active until 1843, when he died quietly in his eighty-ninth year.

Judged by modern standards, Hahnemann's later years deviated markedly from the norms of sound thinking. Certain aspects deserve specific examination.

For effective and rational therapeutics Hahnemann reasoned, quite correctly, that we must know the action of the drugs we use. How can we find out? As already mentioned he devised a rigorous technique of his own which was elaborated in great detail.[23] The subject who tested the medicines (Hahnemann used the term "prove" in its literal meaning) noted and recorded accurately all the "symptoms" resulting from the drug. All states of consciousness which occurred after taking the drug were considered caused by that drug. How can you distinguish a drug-induced symptom from a state of consciousness independent of the drug? Hahnemann did not say. The experimental subject paid the most minute attention to his sensations, kept a detailed record for days on end, and assumed that all his sensations were symptoms of drug action. Hahnemann said, baldly, "All the sufferings, accidents and changes of the health of the experimenter during the action of a medicine . . . are solely derived from this medicine, and must be regarded and registered as belonging peculiarly to this medicine."[24]

Ipse dixit. The faithful cannot argue or question. As a result, reams of paper were devoted to recording the symptoms "produced" by drugs. A garrulous person who introspected carefully for several days after taking a single dose of medicine could "discover" an amazing number of symptoms, all minutely recorded in homeopathic materia medica. Thus, carbonate of lime produced over a thousand symptoms. Some of these were "shooting and drawing pains in the limbs, chiefly at night," "on walking in the open air, sadness, with tears," "snoring during sleep," "dreams frequent, vivid, anxious," "disposition to weep, even about trifles," "dizziness after scratching behind the ear," "purulent discharge from the ears," "fetid smell from the nose," "incarceration of flatulency," "wetting the bed," "prolapsus uteri," "warts on the hands," to mention but a few.[25] If any patient experienced such symptoms in the course of spontaneous disease, carbonate of lime, by inducing the symptoms artificially, should cure them.

Enlightened homeopaths, such as Robert Dudgeon, realized the absurdity to which this could go, and ultimately, of course, all this extravagance collapsed under its own weight. Meanwhile it provided ample targets for the opponents of homeopathy. The significant phenomenon, however, is not the absurdity of the writings but the very existence of these writings and "provings," not the valid criticisms of contemporary wits but the very existence of the criticized doctrines. To this point we will return.

Hahnemann's *Organon* was the bible of homeopathy. The fifth (and last) edition, published in 1833, represented the full development of his calcification. Completely dogmatic, it made pontifical announcements and wide generalizations but only rarely bothered to give the supportive evidence. The major tenets we have already considered from his earlier works, where the origin is clear. In the *Organon* there is much subtle logical elaboration, with systematic development, not especially profitable to take up here. The critics of homeopathy stressed two particular aspects: the emphasis on high dilutions and the doctrine of psora.

As he grew older and receded more and more from the real world, Hahnemann depended more and more on infinitesimal doses, the most vulnerable part of homeopathic doctrine. In Section 270 of the *Organon* he gave directions for preparing a homeopathic remedy, while elsewhere in the volume he explicates the various principles:

Take two drops of fresh vegetable juice mingled with two drops of alcohol. Dilute this with ninety-eight drops of alcohol, and "potentize" with two shakings (or "succussions"). Potentiation is a peculiar process, not at all clear in its workings, whereby the full flavor, the essence, or the full goodness or efficacy of the substance is brought out. A substance is truly active only if potentized, and, conversely, if not potentized, its medicinal effect will be in abeyance or, as he says elsewhere, "The spirit of the medicine, so to speak, becomes much more unfolded, developed, and rendered much more penetrating in its action."[26] It is, perhaps, analogous to the claim that only toasting will bring out the real flavor of a tobacco. At any rate, the first dilution served as a starting point. Then there was prepared a series of twenty-nine other phials, each with ninety-nine drops of alcohol. From this first phial a single drop was added to the second phial, which was shaken twice, and then serial dilutions carried out for all thirty containers. The last one was the thirtieth development of power, which he called the "potentized decillionth dilution," and the one which, in his old age, he recommended as most desirable and effective.

He declared that these smallest conceivable doses affected the patient "in an incredible degree" and scornfully remarked, "How insignificant and ridiculous is *mere theoretical* scepticism in opposition to this unerring, infallible experimental proof,"[27] the "proof" consisting of his mere assertion. And in a footnote he declared, ". . . the homeopathic medicine becomes *potentized* at every division and diminution by trituration or succussion!—a development of the inherent powers of medicinal substances which was never dreamed of before my time."[28] And he related how previously he used to use ten succussions, or shakings, at each dilution, but this made the medicine too powerful, so that he had to restrict himself to only two shakes at each dilution.

It is no wonder that critics like Holmes or Simpson fairly retched at this sort of maundering. But even worse was the ultimate imbecility. Instead of solutions, he recommended "globules," that is, very fine particles of sugar (lactose), so small that three hundred such globules would be adequately moistened by a single drop. One globule, then, containing a three-hundreth part of a drop of the thirtieth potentized dilution, was an effective dose if placed upon the tongue. It was effective even if merely smelled. Such a globule, dry, placed in a small, tightly corked phial might be sniffed repeatedly, much as our present-day nasal inhalators. Said Hahnemann, a globule of this sort retains all its power *undiminished* for at least eighteen to twenty years, even though the phial be opened a thousand times during that period. However, he insisted that it be protected from heat and sunlight.[29] Evidently these forces exerted a stronger magic than did his own magical preparations.

Critics have amused themselves and their readers by long mathematical calculations to show the degree of attenuation. Thus, Worthington Hooker claimed that if the population of the world remained constant and if homeopathy had started with Adam and if every person took three or four drops daily, it would take a *sextillion* years to use up one grain of medicine carried to the thirtieth dilution.[30] Furthermore, if dilutions were made in the proportions of one to one hundred, without discarding anything, one drop with which we start would be mingled with one hundred pints by the third dilution, with ten billion gallons by the ninth, with a volume of fluid greater than the earth by the fifteenth, and, by the eighteenth, with a volume greater than the sun. And there would still be twelve more dilutions to go. It is fortunate perhaps that Hahnemann was not acquainted with the googols and googolplexes of modern mathematics.

James Simpson, referring to these claims, declared that it was a waste of time to try to refute them by reasoning.[31] He was right. The true believer is not swayed by his cerebral cortex. *Credo quia absurdum* is not restricted to religion.

There is a bit of doggerel on the subject which should be rescued from oblivion:

Take a little rum
The less you take the better;
Pour it in the lakes
Of Wener and of Wetter.

Dip a spoonful out,
Mind you don't get groggy,
Pour it into lake
Winnipissiogee.

Stir the mixture well,
Lest it prove inferior,
Then put half a drop
Into Lake Superior.

Every other day
Take a drop of water,
You'll be better soon,
Or at least you ought to.[32]

Even Hahnemann himself could not exclude the obvious fact that very often the homeopathic treatment did not cure the patient. Improvement there might be, but all too often it was only temporary. Now, at this period of his life Hahnemann dealt very little with acute ailments (which frequently, of course, got better by themselves). He did not make house calls but saw patients only in his own home at certain specified hours. Those who consulted him, therefore, were ambulatory, suffering chiefly from chronic ailments. Gradually realizing that such patients were not responding adequately, he began to revise his doctrines and amplify his theories. In 1828 he published his monumental four-volume work, *The Chronic Diseases,* which announced certain new principles. Hahnemann by this time was an old man. His mind was clear, but his judgment left much to be desired. The new doctrines, although enthusiastically received by some of the faithful, offered his critics and enemies much opportunity for ridicule, for the new "insights" were getting more and more fanciful.

He divided chronic diseases into the artificial and the natural. The artificial diseases resulted from improper treatment, that is to say, from the medication of the hated "allopaths." Ordinary doses of drugs, as well as venesections and other surgical procedures, might hopelessly weaken the patient's "vital force," with unhappy outcome. Such "inroads on human health effected by the allopathic non-healing art . . . are of all chronic diseases the most deplorable, the most incurable."[33] This doctrine is, of course, a superb defensive weapon which renders the entire homeopathic system logically airtight, almost invulnerable. If any patient had previously received "allopathic" treatment, and if subsequent homeopathic remedies then failed to cure, the reason is clear: *the previous allopathic remedies had set up serious chronic disease which was incurable.* If, however, homeopathic treatment was successful, that is, if the patient recovered, then the chronic ailment had been reached in time. There would thus be a double triumph, once over the original condition, once over the medically induced exacerbation.

Apart from the medically induced ailments and conditions resulting from faulty habits, there were the true, natural, chronic diseases. These, Hahnemann taught, arose from miasmata, that is, they were infectious. All true chronic diseases arose from one of three sources: from syphilis; from "sycosis," by which he meant the "venereal warts," or condylomata acuminata; or from psora. And of these three by far the most important was psora. Syphilis revealed itself, he taught, by the venereal chancre; sycosis, by the local cauliflower growths. But from psora came the overwhelming number of chronic diseases which masqueraded under many different appearances. Among others, Hahnemann listed such diverse states as hysteria, imbecility, convulsions, scoliosis, cancer, gout, hemorrhoids, jaundice, dropsy, amenorrhea, hemorrhages, asthma, impotence, urinary calculus, paralysis—all these and innumerable others, hitherto considered independent diseases, were only manifestations of the itch. The basic psoric miasma yielded these diverse symptoms because of the wide variations in human constitutions and the multiplicity of environmental factors.

It was quite easy to claim that all chronic ailments stemmed from the itch. If the patient had ever in the past had a skin eruption of any sort, that was presumptive evidence that the condition was scabies. And if the patient did not remember any skin eruption? Well, the miasm was so highly infectious—if the "virus" so much as touched the skin, the disease was contracted—that the original infection might have occurred in infancy or some other unremembered period of the patient's life. Against this type of reasoning any argument is futile. If we ignore all the differences between one chronic ailment and another, if we arbitrarily assign them all to a single underlying cause, if we assert that cause to be present even without any evidence, then our powers of explanation are unlimited.

To Hahnemann any skin condition whatever proved the existence of psora. And, if the skin disease cleared up after local application, this meant not that the disease was cured but that the virus had merely been driven internally, to break out later. Moreover, following a skin eruption, any other ailment that later supervened was a "secondary manifestation" of psora, a breaking-out of the concealed virus. Hence, it was quite wrong ever to employ local applications, for at best the physician, while seeming to help, would only drive the ailment deep within. Only internal remedies could attack the root, the miasma itself. Freeing the local part from symptom did not diminish the disease but only insured further difficulties some place else.

It was only the faithful who joyously received Hahnemann's announcements. Only the faithful could equate all chronic or recurrent illness with scabies. The more enlightened homeopaths joined other enlightened physicians in ridiculing the whole doctrine, especially since the itch-mite, the accepted cause of scabies, was by this time well known. Dudgeon, the leading British homeopath, wrote, "Had Hahnemann proposed to have ascribed all diseases under the sun to the influence of the moon, I believe a certain number of his disciples would have started up in ecstasies at the brilliant notion."[34] For a brief while the antipsoric remedies became quite the rage among certain practitioners, but the concept gradually receded into the background.

While the doctrine of psora furnished great merriment to the opponents of homeopathy, Hahnemann was pathetically invulnerable to logical criticism. Inconsistencies and discrepancies and contradictions bothered him not at all. Even the simplest critical judgment would perceive the absurdities that Hahnemann had massed together. But the old man was quite oblivious to critical judgment, and after his death his more rabid disciples were equally indifferent. The orthodox followers regarded Hahnemann as the fountain of truth and compared him with all the great benefactors of mankind whose teachings were originally scorned and derided, only to be eventually accepted.

Although progressive homeopaths retained a critical spirit, all too many homeopathic practitioners held literally to Hahnemann's every word, and many disciples even exceeded him in extravagance. The regular physicians, whom Hahnemann called "allopaths," repeatedly pointed out the absurdities. But we may well ask, If homeopathy was so absurd, so contrary to all reason or common sense, why bother? Today no one writes witty essays that the oxcart is a less efficient mode of travel than the automobile. Under ordinary circumstances the superiority of the one is self-evident. *Res ipsa loquitur,* and polemics are unnecessary to establish the point. Obviously, in the middle of the nineteenth century the superiority of regular medicine over homeopathy was *not* self-evident.

Homeopathy flourished because many sick people, following a homeopathic regime, got well. Hooker, with great logical acumen, pointed out that cures following homeopathic treatment did not necessarily result because of that treatment. And cases where homeopathic treatment succeeded after allopathy failed, did not prove the superiority of homeopathy. They showed only that homeopathy was better than *bad* allopathy. Hooker emphasized that, in treating disease, doing nothing was better than doing something badly.[35]

There is one severe flaw in Hooker's reasoning. We will all grant, as an abstract principle, that doing nothing at all is much better than doing something positively harmful. Much of early nineteenth-century treatment was positively harmful. But Hooker failed dismally to indicate just what part of allopathy was bad. It was not at all helpful to say, "Some of our treatment actually harms the patient, and in these cases do-nothing homeopathy is better." This might have been true, but unless he knew *what* treatments were harmful, until he and his fellows altered those treatments, homeopathy quite rightfully flourished. Hooker was in a very uncomfortable position. He knew that homeopathy, for which there was no logical rationale, seemed to help many patients whom allopathy failed. He knew that much of allopathy probably was not at all helpful, if not definitely harmful, but he could not identify the particular harmful aspects and so he could do nothing about it. If he could have said, "*This* allopathic treatment is worse than useless and only encourages quackery," he would have been on solid ground. But to gibber helplessly that *some* allopathic treatments, *unspecified,* are useless, does not advance the cause of medicine. Nor is it helpful merely to demonstrate the absurdities of homeopathic doctrine, if allopathic medicine cannot conclusively demonstrate its practical concrete superiority.

Allopathic errors do not establish the truth of homeopathy. But until allopathy corrected its errors, homeopathy, false as it might be, would attract support. Hooker, in criticizing homeopathy, pleaded for sound thinking, for better scientific method, for more attention to the principles of evidence. Very wisely he pointed out that greater insight into the rules of evidence and the concepts of valid method would lead to the rejection of homeopathy. But it would have another result, for it would also lead to the rejection of "a large portion of the observations contained in the annals of medicine."[36] The errors of homeopathy, he declared, might serve to direct attention of medical men to their own "lesser" errors, thus admitting that conservative or regular medicine was not without sin. Indeed, credulity, facile generalizations, reliance on insufficient data, conclusions drawn from guesswork rather than from evidence, these failures constantly beset regular medicine as well as homeopathy. Hooker recognized this, at least obliquely, and his humility would have been helpful if more widely adopted. Quackery and imbecility flourish when more orthodox procedures do not satisfy. It is precisely the weaknesses of medicine that allow cultism and extravagance. Criticism of such extravagance is desirable, but it should go hand-in-hand with self-criticism.

To cast up a balance sheet on homeopathy, we must realize that Hahnemann belonged, first and foremost, to the eighteenth century. He shared its limitations and prejudices, its attitudes and habits of thought. Although he lived and worked actively for over forty years in the nineteenth century, he never progressed beyond his early environment.

Homeopathy arose as a reaction against barbarous eighteenth-century therapy. Hahnemann sought a rational therapy wherein specific drugs could be utilized against specific diseases. The first requisite was a sound understanding of the actions of each drug. In Hahnemann's formative years such knowledge was not available. To achieve such a goal required a true experimental pharmacology. Part of the goal was to eliminate the absurdly complicated prescriptions which mingled diverse substances and gave no scope for testing the pharmacologic action of any one of them. Against this blunderbuss therapy Hahnemann rightly struggled.

His attempts at a true experimental pharmacology were well-meaning but unfortunate. His approach was uncritical and futile. A greater man would have pioneered new methods for achieving his goals, but Hahnemann had no scientific training, no respect for facts. He fell back on rather silly introspective analysis, the so-called provings, which ended in total absurdity. The intent was good, but it yielded only a monstrous abortion. Hahnemann was not equipped for the gestation of any scientific concepts.

Hahnemann did stress the importance of a detailed history. To understand a patient's ailment the doctor had to inquire minutely into the various symptoms. To get the true picture of disease, Hahnemann taught, the physician must listen not only to the patient but also to friends or relatives who might add details the patient forgot. At first the physician was merely to listen, carefully writing down all that was said. Afterward he could interrogate, to establish various particulars, to determine precise details. "What kind of pain, what sensation exactly, was it that occurred on this spot? Where was the precise spot? Did the pain occur in fits and by itself . . . ? Or was it continued, without intermission? How long did it last? At what time of the day or night, and in what position of the body was it worst. .. ?"[37] This line of questioning has a completely modern ring and could be adapted, verbatim, for present-day medical students learning proper history-taking. But in Hahnemann's day this emphasis was relatively novel.

Here again, although his idea was good as far as it went, its merit was distorted by faulty theory. Hahnemann believed that illnes expressed itself in symptoms; if we knew all the symptoms we would know the complete illness in all its details. For Hahnemann a symptom is what the patient can describe. What modern medicine calls "signs," that is, physical manifestations which are discovered by objective observations but which do not appear in the patient's awareness, these Hahnemann blandly neglected. Hence, while he emphasized taking a detailed history, he totally neglected an adequate physical examination. In this respect he was part of the eighteenth-century tradition. In the nineteenth century, when physical examinations became more and more important, Hahnemann could not move with the times.

In another respect Hahnemann fared somewhat better. He insisted on treating each patient as an individual and strongly opposed the notion that a patient simply exemplified a disease. This emphasis, that each case must be studied strictly individually, was important in his undoubted therapeutic successes. Then as now, when the patient felt that his doctor took a strong personal interest; that his own case was unique; that his doctor, appreciating this uniqueness, determined the treatment individually, such feelings were powerful stimuli to recovery.

Specific homeopathic doctrine was all a sad mistake. A single experiment, in 1790, inadequate and uncontrolled, was its foundation. On the basis of this one experiment Hahnemann erected a huge theoretical superstructure, criticism of which is now quite unnecessary. To be sure, the doctrines of *similia similibus* and of minute dosages do find support in the treatments of allergy. The principle of desensitization depends on minute amounts of the very same agent that causes the illness, but there is no need to debate the amount of credit due Hahnemann because of the phenomenon of desensitization.

The essential tragedy of Hahnemann is simply this: He was completely unsuited, by training or by temperament, for the role that he essayed. He was a classical scholar but in no sense a scientist. He had very little contact with sick people: reading classical authors, studying numismatics, or translating chemistry texts do not furnish adequate basis for sound medical theory. And his actual clinical experience was very slender indeed. He was an excellent scholar, highly intellectual, with tremendous drive to make a successful doctor, but with no talent for medicine and very little actual experience when compared with other medical leaders. His drive for recognition and success turned him into a medical fanatic, incapable of rational judgment or critical analysis. Had he received some sort of recognition before 1790, had he avoided grinding poverty and repeated failure, quite probably he would have remained in closer contact with reality. But, as it was, he followed his own road, oblivious to criticism, neglecting the discoveries of others who were not of his way of thinking. Some good ideas he had, but he was incapable of bringing them to fruition.

Had he lived early in the eighteenth century he probably would have elaborated exactly the same doctrine, but then he might have had even more success and would have aroused far less opposition. His system would have competed with those of Boerhaave or Stahl or Brown, and then, with the advent of the nineteenth century, would have sunk equally into desuetude. His tragedy was that he lived too late, that he was an anachronism unable to adapt to the rising tide of scientific knowledge. Even as an anachronism he was reasonably successful, not because his doctrines were true, but because he battened on the decaying parts of regular medicine, upon the errors and stupidities which his opponents committed. Homeopathy arose out of eighteenth-century excesses. Despite great increase in knowledge these excesses were not adequately corrected for a long time. Hence homeopathy prospered until it was submerged by the improved regular medicine. Error thrives because truth is not sufficiently self-evident.

NOSOLOGIA METHODICA
SISTENS
MORBORUM CLASSES
Juxtà Sydenhami mentem & Botanicorum ordinem.

AUCTORE

FRANCISCO BOISSIER DE SAUVAGES
Regis Conſiliario ac Medico, in Monſpelienſi Univerſitate Medicinæ, olimque Botanices, Profeſſore Regio; Academiæ Scientiarum Monſpelienſis, Londinenſis, Upſalienſis, Berolinenſis, Florentinæ, Phyſico-Botanicæ, Suecicæ, Naturæ curioſorum, & Inſtituti Bononienſis Socio.

Editio ultima, auctior, & emendatior.

TOMUS PRIMUS.

Si morbi cujuslibet hiſtoriam diligenter perſpectam haberem, par malo remedium numquam non ſcirem adferre. *Sydenham.*

AMSTELODAMI,
Sumptibus FRATRUM DE TOURNES.

MDCCLXVIII.

VII

Nosology

Nosology means the classification of disease, something we now take so much for granted that we do not even think about its origin. Today we quite expect our medical textbooks to have detailed tables of contents with explicit headings and subheadings. Or we use that formidable and truly canonical volume, the *Standard Nomenclature of Disease,* which orders and systemizes all diseases so admirably. We forget that this orderly arrangement did not arise spontaneously. It represents a slow, painful development which reaches back to the eighteenth century and has suffered many checks and false starts before attaining its present-day precision.

We can appreciate progress only when we are familiar with what went before. If we glance at seventeenth- or early eighteenth-century texts, we find tables of contents, to be sure, but the separate subjects are merely strung together, one after another, like random beads on a string. There is no consistent guiding principle to lend coherence to the individual topics. By contrast a modern medical text describes diseases in such a way that the groups and subgroups seem to *belong* together, sharing certain qualities which link them to each other.

All this is especially important in science. In 1583 the great botanist Caesalpinus declared that all science "consists in the collection of similar and the distinction of dissimilar things."[1] This is a very narrow viewpoint about science, one which no one today would maintain, but it does have a decided element of truth. It shows the trend of sixteenth-century thinking and carries out that crude empiricism which Francis Bacon advocated: collect examples, collect raw material, collect data, collect *facts*. And then classify them according to their various similarities. This was deemed necessary for proper understanding, representing, as it does, the initial steps of those procedures which have developed over the centuries to the modern scientific method.

Our notions of classification stem from Aristotle, whose concepts permeate our modern thinking. Whenever we have a lot of things—whether they be postage stamps or plants or rocks or books or diseases—sooner or later, when we have enough of them, we divide them into groups or classes. A certain number of our entities will share some particular quality. We therefore place them aside in a special class, every member of which has that common feature.

Aristotelian logic holds that, while there may be many similarities among members of a group, there will be one basic, fundamental feature, most real, significant, true, and important, which is called the *essence*. Every group or class has an essence that expresses the true underlying nature of that class. Any individual who has this essence is a member of that class, and without the essence he cannot belong to that class, regardless of what other similarities there may be. Classes are arranged in a hierachy, of which the lower is the *species* and the higher is the *genus*. A genus in one context may itself be a species of some higher, more inclusive genus. For each class there is a *definition,* which has two elements, namely, the *genus* (that is, the immediately superordinate group) and the *differentia* (essential differentiating feature which characterizes that class). Only when we know these two details can we have a clear notion of the class in question, that is, can we grasp the essence. It is not easy to give a "correct" definition, for the essence may be elusive.

Aristotle, for example, defined man as "a rational animal." "Animal" is the genus, but there are many different species of animals—man, goat, horse, and the like. How does man differ essentially from those other animals? Aristotle thought it was by virtue of his reason. Man, and man alone, is rational, and this expresses his truest, deepest, most real nature. Accordingly, since rationality is the essence, whatever creature is not only an animal but also rational is necessarily a man, and all members of the class "man" can be so characterized. They are *animals* (the genus) and they are *rational* (the differentia).

Now all this may seem to us like the veriest logic-chopping, fit only for academic classrooms. But in the Renaissance scientists began to observe nature more closely and to accumulate a tremendous mass of data. Good scientists do not leave their data lying around in loose undigested lumps but try to bring order and harmony into the material. For this, classification is required. Botanical science at that time was a splendid example of this need. Keen observers noted innumerable "facts" and gave innumerable descriptions, but all their data cried aloud for some organization. Hence arose the taxonomists, who noted the similarities and differences among plants and classified the vegetable kingdom. By so doing they sought to identify the true inner nature of the plants. That is, they sought the *essence,* which, they assumed, was real and independent. All that the scientist had to do was to find it and embody it in a definition.

An important presupposition influenced the majority of thinkers: Genera and species, with their own essential characters, existed in their own right, so constituted by God or nature. Genera and species, they thought, were *natural,* independent of the way man regarded them, owing existence not to classification or mental gymnastics performed by the biologists but solely to Nature, or Infinite Wisdom, or whatever name we choose for the ultimate reality. According to this view we must *discover* the diagnostic marks of the genus; we cannot *invent* them. The genera were given by nature, and "since we are not the governors of Nature, nor can create plants according to our own conceptions," we must "submit ourselves to the laws of nature, and learn by diligent study to read the characters inscribed on plants."[2] This point of view is important in understanding the nosologists, in appreciating what they were trying to do. The taxonomist in the plant world tried to identify the features characteristic of genera and species, and his success was proportional to the degree of insight he had into nature's secrets. To make a successful classification he needed sharp, clear definitions, which would distinguish one group from another without overlap or confusion. So it was with the nosologists, who tried to do for diseases what the botanists had done for plants—find the "natural" divisions which obtained among diseases, discover the real essence, and embody this essence in a suitable definition.

For successful nosology there must be a very considerable amount of information. The great Sydenham, living wholly within the seventeenth century, did not attempt the task. There was not enough material to work with, nor were boundaries and differential features sufficiently defined. Sydenham realized this fully but was very sympathetic to the concepts of nosology. He declared that diseases should be reduced to their species with the same care that botanists used in studying plants.[3] Diseases, he pointed out, might resemble each other in many respects yet be quite different one from the other. It was error, he declared, to treat dissimilar conditions as if they were the same.

Obviously, conditions which are unlike should not be treated as if they were the same. But the problem is a little more complex. Almost any patient could have some features in common with another patient. Both might have a fever, both might have a headache, both might show weakness or pain or a skin rash. The question is not whether there are points of likeness between two sick patients—for we can always find some points of similarity. The important question is whether both patients are alike in certain *essential* features which are so clear that they will sharply discriminate one condition from another. Such an analysis presupposes that a disease does have an essence and that we can discover that essence.

Sydenham pointed out that with plants, if we describe a single member of a species, the description will apply to all members of that species and that the same type of similarity would apply to disease. "Nature, in the production of disease, is uniform and consistent. . . . the selfsame phenomena that you observe in the sickness of a Socrates you would observe in the sickness of a simpleton."[4] We therefore can identify diseases by enumerating the "peculiar and constant phenomena" rather than the "accidental and adventitious ones."[5]

Plants and animal species possess a fairly obvious identity, but diseases are not obvious. Sydenham mentioned the quartan ague as forming a distinct, clear-cut species. We have no difficulty in attributing a plant to a definite species; we can apply exactly the same reasoning to a case of quartan ague. There is the same regularity, uniformity, or, in modern terminology, the same predictability. What applies to one case applies in essence to every case. Nosologists of the eighteenth century tried to extend this circumscription to all diseases. Sydenham, however, did not concern himself with the higher echelons of classification. He was content to bring more precision into a few disease species, leaving more sweeping classification to his successors.

The greatest figure in taxonomic botany, a scientific giant who greatly influenced medicine as well as botany, was Carl von Linné, or Linnaeus, born in Sweden in 1707. Very early in life he evinced great interest in botany. His father wanted him to become a minister, but, despairing of his ever achieving this goal, thought to apprentice him to a tailor or a shoemaker. The town physician saved Carl from these trades, taught him physiology, and encouraged him to go to the University of Upsala. There he not only achieved brilliant successes in botany but also took a degree in medicine. After extensive travels and writings he started to practice medicine in Stockholm in 1738. In 1741 he was appointed to the chair of medicine at Upsala, but he soon exchanged that for a chair of botany, since botanical science gave full scope to his passion for arrangement and classification.

Many eighteenth-century physicians classified diseases, but some efforts were more significant than others. Linné, through his botanical work and his enormous experience in pure taxonomy, gave tremendous impetus to the whole movement, although, when applied to medicine, his technique was not entirely happy. His little volume, *Genera morborum,*[6] published in 1763, exhibits interesting merits and defects.

Linné wrote an extremely brief preface, explaining what he was trying to do. It was obviously important to distinguish one disease from another. This could be done, he thought, on the basis of either causes or effects, or signs. Botanists, by systematic arrangement and definition, could keep plants clearly separate and thereby avoid confusion. Physicians, by imitating their procedure, could achieve an equally clear knowledge of diseases by reducing them to classes, orders, genera, and species and defining them appropriately. He went on to say that whoever mixed up causes and signs would not accomplish his purpose. When we examine Linné's results, we find that his analysis of disease depends not on causal factors but entirely on the symptoms or signs. Using these as a basis, he tried to group diseases in such a way that all those which "naturally" belonged together would be in a single group, arranged under various headings and subheadings.

He achieved eleven major groupings. These he enumerated with their essential and characterizing definitions:

I. Exanthemata (*Exanthematici*): Fever with spotty skin eruptions
II. "Critical" fevers (*Critici*): Febrile illness wherein the urine shows a red sediment
III. Inflammatory fever (*Phlogistici*): Fever with a hard pulse and local pain
IV. Painful diseases (*Dolorosi*): Sensations of pain
V. Mental disturbances (*Mentales*): Derangement of judgment
VI. Illness of quiescence (*Quietales*): Loss of power of movement
VII. Involuntary movements (*Motorii*): Presence of involuntary movements

VIII. Suppressions (*Suppressorii*): Impediments to external passages
IX. Evacuations (*Evacuatorii*): Evacuations of fluids
X. Deformities (*Deformes*): Altered shape of the solids
XI. Blemishes (*Vitia*): Externally palpable lesions

These eleven classes allowed further grouping. The first three all concerned febrile diseases and were so designated. The remaining eight he called "temperati." This word, signifying a proper measure or proportion, is unintelligible until we realize how he defined a fever. Linné stated, baldly and categorically, "A fever is distinguished by a rapid pulse." Hence the "morbi temperati" are those with a moderate pulse, and the term can properly be translated "non-febrile." This aggregate of all the non-febrile ailments he arranged according to the constitutent parts affected. He distinguished the nerves, the fluids, and the solids along with the diseases affecting each. In his over-all analysis Linné presented the following outline, in which the Roman numerals refer to the categories enumerated and defined above:

DISEASES

- Febrile (I, II, III)
- Non-febrile
  - of nerves
    - sensation (IV)
    - judgment (V)
    - movement (VI, VII)
  - of fluids (VIII, IX)
  - of solids
    - internal (X)
    - external (XI)

Within this framework Linné enumerated and briefly defined 325 conditions, each of which he regarded as a separate disease, or genus.

Let us examine some of these classes. The preliminary division into febrile and non-febrile is plausible, but the further analysis encounters many snags. The exanthemata he set apart to comprehend all states showing spots on the skin. He included syphilis, which to modern students should not be in this group, and petechiae, which to us is not a disease at all but only a symptom.

The great difficulty that overwhelmed Linné and many other nosologists was the inadequate distinction between a disease and a symptom. He recognized that there was such a distinction, saying expressly that symptoms bear the same relation to disease as leaves and stems do to plants.[7] That is, he implied the modern viewpoint that a symptom is a single element in a broader pattern and that patterns of symptoms, when constant, make up the disease. But he came to grief when he tried to designate a given finding as symptom or as disease.

For example, he called variola a disease, as we would today. He defined it as a condition of erysipelatoid pustules, suppurating, escharotic, desquamating, leaving scars, accompanied by a synochus fever, headache, and lumbago. Of these nine distinct elements, whose conjunction made the disease variola, no less than six he listed elsewhere as disease genera in their own right: pustule, eschar, and scar are diseases in Class XI; synochus fever is a genus in Class II, while headache and lumbago are genera in Class IV. Linné thus had the paradox of one disease, variola, as the conjuction of six other diseases with a few additional features thrown in for good measure. As a botanist he would not have dreamed of defining one plant as the configuration of six other genera, nor would he have placed a given plant in one genus and established its pistils, stamens, and leaves as three other distinct genera. But as a nosologist he was very much less skilful than as a botanist.

We cannot, of course, criticize him because he grouped syphilis with smallpox. They both show spots on the skin, a feature which may with entire logical propriety be used to define a class. How useful this class would prove to be after it was set up is quite a different matter.

The second class, the *Critici,* I have translated "critical," in the sense of "crisis." The term and the definition are remarkably inappropriate for the members which he forced into the class. In this group he placed the familiar listing of the continued and the intermittent fevers, together with their compound forms. His definition, the common thread alleged to run through all the separate fevers, was a urinary sediment that was red and "lateritious." This referred presumably to the reddish precipitate of urates that is deposited when certain urines stand for even a short time—scarcely a useful or exact criterion.

His third class of fevers was fairly straightforward. The members had in common a febrile state and a localized pain. That is, there was a localized inflammation which accompanied the fever. The separate genera within this class he established according to the site of the lesion: meninges, pleura, stomach, liver, kidney, and the like.

Class IV, the painful diseases, was not a happy discrimination. He had classified diseases according to their signs. Certainly pain is a very important sign of disease but scarcely adequate to characterize, by itself, an entire disease entity, that is, a discrete genus. Nevertheless, Linné made toothache, for example, one disease, sore throat another, headache (of the entire head) another, and hemicrania (ache of only half the head) still another. Arthritis, rheumatism, and pruritis were also among the twenty-five genera in this class. It is noteworthy that today laymen use the terms "arthritis" and "rheumatism" when they have pains in the joints and muscles and believe that these terms designate disease entities. But the more sophisticated know that pains in the joints are merely symptoms common to many readily separable states. If, through ignorance, we cannot separate out the various conditions, we are in the position of Linné in the eighteenth century, of the untutored layman in the twentieth.

In many other classes Linné's definitions were singularly inappropriate for the alleged members. In Class VI, which he called *Quietales* and which I have translated "illnesses of quiescence," he used as a definition "loss of motor power" (*abolitio motus*). This clearly applied to apoplexy, hemiplegia, stupor, or syncope but not at all to such included genera as amblyopia, cataract, anosmia, or defects of hearing or of appetite; nor did it strictly apply to lassitude, languor, or asthma, which he considered discrete diseases within this class.

Other classes were more logical. Class VIII, the suppressions, had two separate divisions: disturbances in respiration, such as cough, asthma, dyspnea, orthopnea, and even empyema; and disturbances in excretion, such as constipation, ischuria, or suppressed menstruation. The connecting thread was an impedance to various passages.

Class X, the deformities, showed some curious reasoning. This class represented alterations in the internal solids and had three subdivisions. The *emaciations* included such wasting conditions as phthisis and marasmus. *Swellings* constituted a second subgroup with such diverse members as ascites, hydrocephalus, tympanites, and, surprisingly enough, pregnancy. The third subgroup was the *discolorations,* in which Linné placed together five ailments which have in common a changed color of the skin. Chlorosis and icterus were fairly obvious, exhibiting a green and a yellow color, respectively. Plethora Linné defined as a reddening of the body from distended blood vessels. Cachexia he included because of the pallor, which can be construed as a changed color analogous to red or yellow or green. The fifth member was scurvy, whose clinical signs were well recognized. The only connection with the other four was a supposed *opacity* of the facies. To us today a change in skin color scarcely seems to be the essence of a disease state.

Today, when we base our classification on etiology, all this appears rather strange. But, if we are ignorant of the etiological factors, we must fall back on some other basis. One purpose of nosology was to bring order and system into the study of medicine, to identify diseases with such clarity and precision that everyone would know what was meant . Giving a name and a definition accomplished this purpose to some extent. A further goal of nosology was to exhibit the deeper relationships and inner nature of disease states by grouping together those states that had some essential similarity. This was not quite so well accomplished. The essence, we have seen, was supposedly conveyed in the definition. To give a sound definition requires great insight, and, in so far as the definition is bad, the insight is defective. Putting together a group of diseases without paying much heed to the ostensible definition denotes very fuzzy thinking, while giving a trivial definition indicates a poor grasp of the subject. Linné wanted to make a neat, orderly system but lacked the logical acumen and the medical knowledge to reach a satisfactory conclusion.

It is very easy, in the light of modern knowledge, to criticize Linné's classification. The principle trouble was that he knew too little about the diseases he classified and was not at all sure what constituted a disease in the first place. We must remember, however, that he was not aware that he knew so very little. To know that we don't know is already a great step forward, and most eighteenth-century writers had not taken that step. It is as if Linné tried to build a house on a very limited foundation. Some buildings can indeed be constructed without very much foundation, but these structures will be definitely limited in size and stability and must be kept within modest confines. The great difficulty arises from attempting too much, from superimposing a top-heavy structure on an inadequate base.

Nosology owes its greatest debt to François Boissier de Sauvages. A contemporary of Linné, Sauvages was also interested in botany but devoted himself primarily to medicine. When only twenty years old (in 1726) he obtained his M.D. degree, offering the thesis "Can Love Be Cured by Remedies of Plant Origin?" suggesting a botanic interest and a romantic temperament. At twenty-eight he became professor of medicine at the University of Montpellier. Very early he had the idea of arranging diseases by genera and species and in 1731 published a small volume, *Traité des classes des maladies,*[8] which greatly influenced Linné. In 1763, in full maturity, he published his monumental *Nosologia methodica.*[9] It was a deliberate, painstaking classification of disease states, taking up, as well, much symptomatology, therapy, experimental data, autopsy findings, and general theory. He also analyzed certain philosophical principles as applied to medicine.

Sauvages, in the line of Occam, Francis Bacon, and Locke, was ostensibly a nominalist. The disputes between nominalists and realists carried over from medieval times and indeed are still vivid today. The nominalists denied the primary reality of universals and of classes. Sauvages emphasized that these are abstractions, while in nature we do not find genera and species but only individuals. What we call genera or species are names, not things. Of course, names are necessary for clear understanding of things, and if you take away names you take away knowledge. All would be confused and obscure.[10] Today we might say that names have a pragmatic value but no independent reality. This contrasts with the view that classes, representing universals, are real, while the individual derives his reality from his membership in the class.

Sauvages pointed out that to be clearly understood our ideas must be fixed and constant. Hence the need for classification, which depends on definitions that will indicate the distinguishing marks, giving neither more nor less information than is necessary for recognizing the thing in question and discriminating it from all other things. He adopted the Aristotelian concepts of defining by genus and differentia.[11] Sauvages, inheriting a formalized schema of genera and species, classes and orders, then faced the problem how best to circumscribe these groups—how to define them to greatest advantage, arrange them most appropriately. He listed several possible methods.[12]

First, he mentioned alphabetical classification, which, however, was scarcely satisfactory. The reason he gave is surprising. Alphabetical arrangement, he said, places dissimilar conditions together and separates those which are similar, so that by this means we cannot achieve any knowledge of diseases. Here we find a covert falling-away from nominalism. We see the notion, hidden away, that somehow there *are* similars in their own right and that alphabetical arrangement is faulty because it does not find the "real" similarities. Today we would not complain that an alphabetical order keeps us from a knowledge of disease. If alphabetizing a list is convenient, fine; if not, don't do it. But an alphabetic arrangement is quite irrelevant to "reality."

A second possible principle for classification, according to Sauvages, was temporal. Since diseases were chronic or acute and lasted for varying periods, the time span was one way of arranging them. This pinciple he dismissed as inadequate, for there was no "natural limit" between chronic or acute. And in any case the terms were quantitative, referring to "more" or "less." Mere quantitative distinction, mere degrees of duration or intensity, could not create or alter a species. Qualitative differences were requisite.

Arrangement by anatomical site was a third method, whereby division took place according to the seats of disease. In such a schema there might be a preliminary division into the general and the localized, followed by division into head, thorax, abdomen, extremities, each with enumeration of diseases. Sauvages condemned this method as confusing, fallacious, and inconvenient. The different elements within a region, the muscles, nerves, fibers, blood vessels, or fluids, are all intimately mixed. That one element is affected does not mean that another part is or is not affected. No limits can be drawn. And, needless to say, without limits we cannot have good classification. Furthermore, diseases are not limited to any one region. The seats of disease are multiple. What, for example, is the seat of vomiting? Is it the mouth or the esophagus or the stomach? Or which membrane of the stomach—villous, nervous, or muscular? Or is it perhaps the intestine? Then, there is bilious vomiting from the liver, or urinous vomiting from diseased kidneys. We cannot localize a disease to a discrete part. Moreover, one part can be affected by many different conditions, and disease in one part can affect other sites. There is, in brief, no clarity of knowledge.

Sauvages here mingled acute criticism with great naïveté. He touched the very spot which is still irritating in present-day textbooks: regional classification is fallacious because diseases are not confined to discrete regions. Today, however, while recognizing the fallacy, we are not too disturbed because there is compensatory convenience which is rather satisfactory. But Sauvages seems to have been haunted by a specter. Despite his nominalism, he seems to say that there *is* a natural order, a real pattern, and that this pattern is a matter of genera and species, that there is a right way and a wrong way, and, if we don't put the "naturally" related diseases together, then the classification is bad. He could not quite maintain the nominalistic viewpoint, that there are no genera and species at all except in so far as we create them, that there is no *really* correct way to categorize disease apart from practical or logical convenience. In other words, good or sound classification depends not on correspondence with natural genera but on logical criteria. Sauvages, professing nominalism, floundered on this point.

Having discarded anatomy as a guiding principle, Sauvages discussed etiology. This also he showed to be most unsatisfactory. If we try to define and separate diseases by their causes, we are overwhelmed: causes are largely unknown, or, if asserted to be known, are "hypothetical" (that is, a matter of speculative fancy without sound evidence). Medicine, he insisted, could never progress except on a foundation that was firm and indisputable, not changeable or inconstant. That etiology could not furnish such a sound basis seemed obvious; he merely pointed to the leading writers whose assertions about causation differed so markedly one from the other. Etiology, as understood in the eighteenth century, was scarcely a firm foundation.

At this point we must glance at a few principles basic to eighteenth-century medical thought. *Certainty* attached only to what was immediately sensible. What was "hidden," that is, inferential, was not certain and therefore not very valuable. A *cause* was never given in the senses. Causes were hidden, remote, or inferential, valid only as they furnished some characteristic or distinguishing mark immediately perceptible. When arranging diseases, we should use these directly perceivable marks, not the inferred or hidden or "hypothetical" causes.

Since etiology, as he understood it, was very vague and unsatisfactory, Sauvages, like Linné, relied on the symptomatic method to classify disease. Sometimes historians accuse Sauvages of considering symptoms as diseases and then merely subdividing symptoms into minutiae. This is not a fair criticism, since it quite neglects the contemporary concepts. A symptom, to Sauvages, was anything directly perceived, either by the patient or the physician. It included our present-day "symptom" and "sign" all rolled together without distinction. A symptom was a mark, or characteristic, of something else. He said specifically that lesions or changes, in so far as they were perceptible, were symptoms.[13] Sauvages' great plea was merely this: we should classify diseases on the basis of directly observable changes, or symptoms, not on the basis of hypothetical or inferential or imagined changes (that is, so-called causes).

Once we define a genus, we can then distinguish the various species which bear not only that common generic character but differentia of their own. Sauvages declared that as many species might be enumerated as there were similarities among the sick individuals of that genus. At this point he gave up the purity of his doctrine. He declared, further, that for the sake of brevity or usage the anatomical seat, or the predisposing or exciting causes, might be adopted to distinguish species. Having just declared that sound definition did not proceed by either causes or seats, he now turned around to admit these criteria. He did so for greater convenience, since a name which included a seat or a cause would be a shorthand way of expressing a group of symptoms, in modern terms, a syndrome. He accepted, for example, "mediastinal pleuritis" or "dorsal pleuritis" because these had their own symptoms succinctly expressed by the anatomical terms. They had pragmatic value.

With this as a background, we can observe his system in action. Sauvages created 10 major groups or classes, which corresponded substantially to the 11 of Linné. Sauvages had 44 orders and 315 genera, again very comparable to Linné. But Sauvages, not content with genera, continued the subdivision into species. These he regarded as the disease entities, and of these he distinguished a very large number, some 2,400 in all. The text is truly monumental in scope. We can form a reasonable notion by selecting a few particulars.

If we wish, for example, to study his views on sarcoma, we find them listed under the *Vitia.* This class, containing the relatively mild cutaneous states which are amenable to surgery, has seven subdivisions or orders (such as macules or cysts or wounds). The order *Excrescences* comprises the tumors of the solids (as contrasted with the liquids). Its members all grow slowly and do not disappear by suppuration. Within this order we find the genus Sarcoma, the distinguishing mark of which is its fleshy consistency. This is applicable to all the following eleven species defined by Sauvages:

1. *Sarcoma vulgaris,* a painless fleshy tumor, congenital or acquired, increasing in size, which injures by its location and its mass but which does not attack glands. (Such a category would include many lesions we would still call sarcoma, as well as many benign tumors of subcutaneous origin.)
2. *S. narium* (the present-day nasal polyps).
3. *S. oculorum,* fatty excresences of eyelid or outer canthus (probably what we now designate xanthelesma).
4. *S. epulis,* an excrescence in the oral cavity, especially of the gingiva (our present-day epulis).
5. *S. cercosis,* polypus of the uterus, or fleshy excrescence hanging from the uterus or vagina. (This would include, besides simple polyps, the rare tumor we now sometimes call Sarcoma botryoides, as well as certain unusual leiomyomas.)
6. *S. natta,* a pendant tumor of the back, growing to a very large size, causing injury by sheer mass. (Probably this corresponds to massive lipoma of the back, now seen very rarely.)
7. *S. bicephalium,* a tumor of the scalp, which may grow so large as to resemble a second head. (Perhaps this repre-

sents our present-day "turban tumors" or cylindromas or epidermal cysts.)

8. *S. fungosum,* a soft fungating excrescence arising from ulcers and wounds. (This seems to be exuberant granulation tissue.)
9. *S. scroti,* or sarcocele, a painless growth of the testis, different from either inflammation or scirrhus. (This probably referred to primary testicular neoplasms.)
10. *S. varicocele.* (This, Sauvages declared, belonged to varices of the spermatic vessels rather than to the sarcomas. Why, then, he included this under sarcomas is not clear.)
11. *S. encanthis.* (This term he applied to excrescences of the lachrymal caruncle and to chalazions.)

Here, in a single genus, we have numerous growths which all have fleshy consistency. The different species are subgroups whose members have similarities to one another but are distinct from subgroups of other genera. Sauvages established his species largely on an anatomical basis: the nostrils, the gums, the uterus, the scalp, the back, the eye, and so on. The justification for using anatomical sites to distinguish species, he had already discussed. It gave a useful means of identifying a group of symptoms. If we except varicocele, the classification is not without merit. The species are fairly clear-cut and, as clinical entities, are still approximately recognizable after almost two centuries. To be sure, his criterion for the whole genus, namely, a fleshy consistency, is vague and inexact. But real precision, more exact criteria, came only with new technical advances and the new concepts to which these advances gave rise.

Species represented disease states that could be separated out as significantly different from other and possibly related conditions. Any particular disease state deserved a name as a means of identification. The identification could take place only by a suitable definition attaching to that name. Names, when multiplied, had to be arranged and classified to show their interrelationships. This is a procedure we employ today, just as did Sauvages in the eighteenth century. Classification as a practical aid is sometimes very badly done. Sauvages is subject to severe criticism, not because he called symptoms diseases (or because he broke down genera into species), but because he was frequently illogical in his classifications, confused or vague or inaccurate or redundant. Had he with logical rigor maintained his announced principles, he would have done fairly well, considering the knowledge available. Instead, he mixed up many different points of view. His species were often quite a hodgepodge because they lacked any clear defining principles, uniform points of reference, or consistent plans of division.

An example of good classification is his analysis of cystitis. This he placed among inflammatory diseases affecting membranous tissues (similar to pleuritis or gastritis or enteritis). That is, the *genus* cystitis lies in the *order* membranaceae of the *class* phlegmasiae. He described three species: (1) spontaneous cystitis from "internal causes" such as a plethora with acrimony of urine; (2) cystitis from cantharides—he recognized that a specific causative agent, cantharides, might induce cystitis; (3) traumatic cystitis from a variety of agents such as a blow, or contusion, a wound, or a calculus. If we made a modern classification along these lines, we would have three similar general groups: infectious, toxic, and traumatic cystitis. Then we would enumerate varieties under each.

Quite bad, however, is his classification of convulsions. This *genus,* defined as involuntary agitation of the limbs and falling, belongs to the *order,* partial clonic spasms, of the *class,* spasms. He listed sixteen species. Some, eminently sensible, conformed to a sound and logical analysis. But others were quite nonsensical, based on principles entirely tangential one to another and allowing much overlap. Part of his analysis was etiologic. He distinguished, for example, convulsions from inanition (as from excessive bleeding or hemorrhage), and his discussion, taking up experimental studies and clinical examples, established a satisfactory clinical entity. Other species of convulsions based on etiologic factors were those resulting from febrile illnesses, traumas, and toxins. Some convulsions were associated, etiologically, with calculi in the kidney (referring possibly to what we now call uremic convulsions) or with "hydrocephalus" (possibly various forms of cerebral dysgenesis). All these species correspond to etiologic factors and, entirely logical, are quite satisfactory for sound classification.

However, he didn't know when to stop. As he continued the enumeration he lost his guiding principles. We find such items as *Convulsio abdominalis, C. universalis, C. habitualis,* and *C. intermittens,* whose names are self-explanatory. They do not exhibit an etiologic reference but embody some anatomical feature or peculiar or trivial quality. There is thus a cross-classification where, instead of one single principle of division, two or more are mingled indiscriminately. Etiology joins with intensity, anatomical location, or temporal relationship to characterize disease species.

The class *Dolores,* or pains, is a most unfortunate category. The genus Cephalalgia contains thirteen separate species of headache. To attain this number, Sauvages used discordant divisional principles. Some species he established by etiology, as headache from plethora; others by their qualitative character: a pulsating headache was one disease, an intermittent headache a different disease. Other genera in this order show similar faults. Pains in the kidney or pains in the chest are genera whose species derive sometimes from etiology, sometimes from transient qualities.

There are thus two major defects. Sauvages did not maintain a constant principle of classification. Then, he treated one facet of disease (as headache from plethora) as if it were an independent condition. Many of his species are well-drawn clinical entities, but others are trivial aspects. He did not adequately distinguish an aspect from the whole, symptoms from disease. Even today, the distinction is not always clear, but Sauvages was not really aware of the problem.

When we read Sauvages, we feel that a brilliant mind has gone astray. It is as if he were so devoted to classification that, like the Sorcerer's Apprentice, once started, he could not stop. He kept on making distinctions. After he ran out of significant differences, he used very insignificant features. He seems almost to have been padding his text, as if, when he had used up the clear-cut entities, he cast around until he found a few more items to force into the genus. Hence his schemata are partly logical and orderly but often overlaid by distracting and insignificant cross-classification, quite without order. The seventeen species of cough or the twenty-nine species of singultus are not absurd because of their numbers. They are absurd because of their confusion. They mix up etiology and description in senseless fashion. Confusion results when logical principles of simplicity and order are crassly neglected.

Sauvages had many imitators who merely rang some minor changes, but it was William Cullen who first made really significant advances. In 1769 Cullen published a nosology as a bare outline, without explanatory text but showing already a considerable improvement over Linné or Sauvages.[14] Later he wrote his magnificent textbook of medicine, the famous *First Lines of the Practice of Physic,*[15] which followed fairly closely the nosology already published. One of the later editions of *First Lines* has happily combined most of the nosology with the extended text.[16]

Now, Cullen was a brilliant physician and, unlike his two predecessors in this chapter, was not a systematic botanist. He was interested not in pure taxonomic detail but in concrete medical problems, which he envisioned in a manner both logical and practical. Thus, he declared, ". . . the history of disease . . . is far from being complete and accurate; and I say it is the attempt at nosology which chiefly serves to point out these doubts, to start questions and to direct our farther observations."[17] That is, nosology had heuristic value.

Of course, he was influenced by the general format that his predecessors had introduced. He too built his arrangement on symptoms, but he tried to correlate these with functions in a fashion more explicit and deliberate than had Linné. He wanted to simplify, to keep to essentials, and to make these as meaningful as possible. Instead of eleven classes he distinguished only four, of which the first three, Pyrexias, Neuroses, and Cachexias, corresponded respectively to disturbances in the vital, the animal, and the natural functions. This was much preferable to classification according to fluids, nerves, and solids. Cullen, however, was saddled with various traditional distinctions which he could not discard. Of his four classes he treated three in great detail when he wrote his *First Lines.* The fourth class, Locales, containing 60 of the total 151 genera, was more or less a scrapbag into which were dumped many poorly defined, badly conceived genera. This entire class he did not describe at all in his *First Lines.*

Cullen was very sparing in creating genera, or in subdividing into species or varieties. The first three classes, covering the clinically important states, had only ninety-one genera altogether. He did not make distinctions merely for the sake of making distinctions, but the subdivisions had definite clinical significance. They corresponded in general to common sense as well as logic, and his definitions were succinct and clear.

His class Pyrexias included five orders: the *fevers* without localized signs (the familiar intermittent and continued forms); the *localized inflammations,* such as in the throat, lungs, or kidneys; the *exanthemata;* the *fluxes* accompanied by fever, such as "catarrh," and various forms of dysentery; and, finally, a rather discordant order of *hemorrhages,* including epistaxes, hemoptysis, and menorrhagia. The first four are appropriate subdivisions, but the hemorrhages do not seem to belong with the others.

The class Neuroses comprised those various nervous ailments showing weakness or loss of motion in the vital or natural functions. These included such states as apoplexy, syncope, convulsions, chorea, and "madness," arranged in four orders. Much of this is fairly clear, but the order Spasmi, covering abnormal movements of muscles or muscle fibers, seems a little confused. It not only embraced such states as palpitations, asthma, colic, and hysteria, but it also included diabetes, by the following reasoning: "This disease consists in the voiding of an unusually large quantity of urine. As hardly any secretion can be increased without increased action of the vessels concerned in it, and as some instances are . . . manifestly spasmodic, I have no doubt of arranging the diabetes under the order of Spasmi."[18] If we did not know much about diabetes, perhaps this would be plausible.

The third class, Cachexias, we can profitably compare with Linné's Class X, the deformities. Each had three subdivisions, of which the first, in each case was emaciations and the second, swellings. The third subgroup Linné designated discolorations, on the basis of changed skin color. Cullen's third category was *impetigenes,* which he defined as a "depraved habit, with affections of the skin," that is, a systemic disorder manifested by skin changes and distinct from mere local skin disorders. Within this order Cullen placed scrofula, syphilis, and scurvy, as well as leprosy and elephantiasis. The sharper definitions and improved acumen are noteworthy. Interestingly enough, however, Cullen, like Linné, also placed jaundice in the same general group as scurvy.

The fourth class, the Locales, not discussed in *First Lines,* contained eight orders. The whole class was poorly conceived and badly arranged. The only improvement over his predecessors was that all these conditions were bundled together and, so to speak, thrust out of sight. The eight orders covered various defects, losses, deprivations, and sensory disturbances. Such diverse states as loss of appetite and loss of speech were arranged together. Excess flow of tears or of saliva went hand in hand with enuresis and gonorrhea. Various tumors, such as aneurysm, cancer, bubo, or ganglion, also found a place within this class, as did "ectopic" states such as hernia and prolapse. Wounds, ulcers, and fractures were also considered to belong in the class Locales. Cullen's inspirations definitely ran out when he tried to analyze and arrange these states. Most of the arrangements he took over from his predecessors, and he did not exhibit any noteworthy insight of his own.

In some areas, Cullen clearly separated symptoms from diseases. While discussing dyspnea, he explicitly differentiated "symptomatic and idiopathic affections." He distinguished "between those difficulties of breathing which are symptoms only of a more general affection, or of a disease subsisting primarily in other parts than the organs of respiration, and that difficulty of breathing which depends upon a primary affection of the lungs themselves."[19]

Present-day usage equates "idiopathic" with "cause undetermined," but in Cullen's day this was not the case. "Idiopathic" was the antonym of "sympathic." In the former the organ in question was the locus of the disease, regardless of cause. In the latter the organ merely reacted, by sympathy, to disease some place else. "Idiopathic" had no intrinsic connection with causation, known or unknown, but only with the primacy of the disease.

When he listed the idiopathic species, however, he intruded certain causal factors and did not in any way keep to a pure classification. Thus, among his eight species of dyspnea he listed *D. sicca* (dyspnea with cough that is usually dry), *D. terrea* (dyspnea where the cough brings up earthy material), *D. aquosa* (involving hydrothorax), and *D. pinguedinosa* (dyspnea in men who are too fat). By contrast, in his symptomatic species, dyspnea was a symptom of (1) diseases of the large vessels, (2) swelling of the abdomen, and (3) "various diseases."

Although Cullen's distinction between idiopathic and symptomatic was often very badly drawn, it was nevertheless explicit and marked a welcome advance. When in genus after genus we see the "idiopathic" species separated from the "symptomatic," we appreciate the refreshing acumen. Today we would not at all agree with what he designated idiopathic. But the significant feature is not that his knowledge was defective but that he made a sound logical distinction and kept to it fairly well.

Cullen by no means escaped cross-classification. He sometimes mixed up qualities and causes and anatomical loci when separating off various species, but he did not err nearly so much as did Sauvages. His analysis of jaundice offers a fair example of his merits and defects. He defined jaundice as a yellowing of the skin and eyes, with whitened feces and dark red urine which tinted a yellow color anything put into it. Then he distinguished five species: (1) *Icterus calculosis,* with acute epigastric pain and excretion of biliary concretions in the stools; (2) *I. spasmodicus,* painless, following "spasms" and "emotional" change; (3) *I. hepaticus,* painless, following diseases of the liver; (4) *I. gravidarum,* arising during pregnancy and disappearing after parturition; and (5) *I. infantum,* appearing in infants about one day after birth. Here we have five distinct conditions. There is no unnecessary multiplication of entities, no distinctions without a difference. To equate some of these with modern concepts might be difficult, but the arrangement per se is not at all unreasonable.

He avoided entirely that morass of confusion, the class Dolores. Both Linné and Sauvages leaned heavily on this category, but Cullen realized that no sound or worthwhile classification could be based on either the kinds of pain or the loci of pain.

It is interesting and somewhat disheartening to read later criticism of Cullen. Thomas Young, that versatile genius, wrote in 1813 that he found Cullen "replete with such irregularities and inconsistencies . . . that nothing short of the eminent talents of the author . . . could have induced the medical public to tolerate the substitution of such a classification, for the less objectionable ones which had preceded it." And, to be more specific, ". . . his genera, his orders, and his classes, are lamentably deficient in the essential qualities of a logical and systematical method." In brief, the defects are so numerous "as easily to be observed even by a superficial reader."[20] An interesting commentary, less than a generation after Cullen's death. That anyone should prefer a nosology from Cullen's predecessors is most amazing. And Young, as a critic, offered his own private nosology, which is a monstrosity best passed over in silence. Defects Cullen certainly had, but, to present-day critics, his faults were far fewer than his predecessors'.

Nosology became a popular exercise. Many physicians tried their hands at arranging diseases, producing such extravaganzas that the entire movement fell into disrepute. Among those who helped diminish the real value of nosology was Erasmus Darwin (1731–1802), a remarkable figure now known chiefly as the grandfather of Charles Darwin but sometimes remembered as a contender for the title of worst poet in the English language. Erasmus Darwin displayed a powerful mind and a vigorous imagination, strong scientific leanings and occasional flashes of insight, but he was also a man of prejudice and passion who lacked balance and intellectual discipline.

He wrote a large two-volume work on medicine, the *Zoonomia*,[21] of which the first volume comprised miscellaneous essays on selected topics in anatomy, physiology, psychology, physics, and medicine, while the second volume was a classification of disease with descriptive text. The arrangement was strictly by class, order, genus, and species; yet the book marked a sharp departure from the nosologies hitherto considered. Darwin set up his classes according to totally different principles. Instead of grouping by signs and symptoms he tried to arrange by causal factors. This at first glance might seem a most enlightened innovation. We must recall, however, that Sauvages had already flatly rejected causation as a basis, because notions of causation were far too confused in his day to yield a sound nosology.

Darwin attempted a psycho-physiological analysis of behavior, describing four distinct powers or faculties acting on the body. He believed that the "motions" affecting the body might result from: *irritation,* excited by external bodies; *sensation* of pleasure or pain; *volition,* aroused by desire or aversion; or *associations* with other movements. All illnesses, he thought, represented disorder of one or another of these functions. These four faculties served to demarcate the "natural" classes of disease, and the cause of any illness was thus a disturbance of a particular function. Further subdivisions rendered the supposed causal relation more precise. The major functions might exhibit either excess or deficiency in actions or perhaps a "retrograde action."

The excess or deficiency might be manifest in any of several modalities. Thus, all the diseases of irritation represented one "class." Excess irritation was one "order" whose "genera" indicated the different localities. Increased irritative action, for example, might affect the "sanguiferous vessels" or the "secerning [secreting] system" or the "absorbents" or the "other cavities and membranes" or the "organs of sense." These were the five genera of the first order of the first class of diseases. Any specific ailment was then literally a "species" of one of these genera. For example, constipation belonged to the genus "increased actions of the absorbent system," which was its immediate cause. A (warm) sweat was due to an "increased action of the secerning system," which was its genus and its cause. But a hydrothorax had a different etiology—not an excess action, but a diminished action, involving the absorbent system.

Epilepsy was a disease representing a disturbance not of irritation but of volition. The cause was an "increased" volition (Order 1) with "increased action of the muscles" as its genus. Gout or rheumatism were diseases of "association" (Class 4), caused by "increased associate motions" (Order 1) but "catenated with sensitive motions" (Genus 2).

Darwin's guiding principle was that a classification of diseases according to their proximate causes would permit a better understanding of the nature of the illnesses and provide a means for comparing their essential properties. Furthermore, this nosologic arrangement was supposed to facilitate treatment, since most species within a genus, most genera within an order, would require the same treatment.

But the theories constituted a rope of sand. Quite obviously, he owed a heavy debt to John Brown, who held that excessive or defective excitement caused diseases and that treatment was quite simple. Darwin thought that by labeling a disease an excess or a defect of something-or-other he had thereby identified the cause of that disease. He represented the worst of the armchair philosophers who believed they could identify causes by juggling a few words around, by merely rephrasing or restating what was already implied. To say that a catarrh is *caused* by excess secretion of fluid is not very helpful. The catarrh *is* the excess mucous secretion. That is its definition. Simply to restate an already accepted definition, and to call that restatement a cause, is not a very valuable contribution. Compared with the acumen of Pringle or Morgagni it is merely an exercise in obscurantism.

Darwin spun his entire nosology without any regard for empirical support. He could call anything he wanted the "cause" of any disease and was not in the slightest degree bothered by evidence or lack of it. He did not concern himself with diseases in the ordinary accepted sense. His species were usually only symptoms, and sometimes normal functions, and he grouped together the strangest bedfellows. Thus, deglutition, respirations, sneezing, hiccough, yawning, and tenesmus were all species (diseases) of the genus "increased action of muscle." And parturition was in the same genus. In his class 3 we find such "diseases" as vanity, pride of family, ambition, grief, fear of poverty, lust, anger, all within the single genus "increased actions of organs of sense." Such conditions as syncope, venous hemorrhage, palpitation, exuberant menstruation, deficient menstruation, spontaneous abortion, and scurvy, all these were species in the same genus, all "caused" by "decreased actions of the sanguiferous system." It is worth noting that he listed only 474 species in his entire nosology.

Previous writers had used signs and symptoms to define or circumscribe various disease states. Darwin had no such interest. He constructed for himself a neat, symmetrical, logical form. This he used as a mold into which he poured a miscellaneous lot of symptoms, functions, diseases, feelings, and emotions and ended with an artificial structure whose only value is the glimpse it offers into the strange mind of Erasmus Darwin; it bears little relation to objective reality. His *Zoonomia* contains, to be sure, a great many sharp, clinical observations and considerable medical knowledge. By eighteenth-century standards it is often quite sound. But as a contribution to nosology it merely rendered the entire subject quite absurd.

Any study of early nosology must include its archfoe, Benjamin Rush. A man of forceful opinions, forcibly expressed, he maintained the unity of disease. When discussing febrile illnesses, for example, he insisted that there was but one fever, and he would not allow artificial division into genera and species. There is a disposition, he said, for "diseases to anastomose with each other. To describe them, therefore, by any fixed or specific character is as impractical as to measure the dimensions of a cloud on a windy day."[22] Nosologic arrangements, he continued, "erect imaginary boundaries between things which are of a homogeneous nature." Actually, said Rush, he used the plural term "diseases" only from conformity to custom, since "properly speaking, disease is as much a unit as fever."[23] In fact, he said elsewhere:

> the multiplication of diseases . . . [is] as repugnant to truth in medicine, as polytheism is to truth in religion. The physician who considers every different affection of the different systems of the body . . . as distinct diseases when they arise from one cause, resembles the Indian or African savage, who considers

> water, dew, ice, frost and snow as distinct essences; while the physician who considers the morbid affections of every part of the body . . . as derived from one cause resembles the philosopher who considers dew, ice, frost, and snow as different modifications of water.[24]

But Rush, after maintaining the strict unity of fevers (and for that matter of all disease), goes right over into the nosologists' camp by offering his own classification of fevers.[25] It is not a very good classification—in fact, it is very inferior to Cullen's—but it divides and enumerates, relates one state to another, and separates off certain conditions from certain others. Rush sedulously avoided the terms "genus" and "species," but otherwise he did what the nosologists were doing, with only a quantitative difference.

The transition between the eighteenth- and the twentieth-century nosologists lay through Phillippe Pinel, remembered today principally for his pioneer work with the insane. Pinel wrote a three-volume work, *Nosographie philosophique*, which went through numerous editions.[26] Nosography and nosology are by no means identical. The former is the description of disease, the latter, classification. Modern writers concern themselves principally with descriptions, while the eighteenth-century nosologists tried to classify in such a way that the relationship of one disease to another would be fairly apparent from the arrangement itself. Pinel, interested chiefly in description, did not publish any separate nosology at all. Instead, he arranged his text around an approximate nosologic framework.

The details of his arrangement, which belongs properly to the nineteenth century, need not detain us. His system was greatly simplified, with only five classes, eighty genera, and less than two hundred species altogether. At heart Pinel was not a nosologist, and, although he kept the nomenclature, perhaps he did so only because it was still fashionable. Certainly he was very far removed from the Aristotelian tradition, which tried succinctly to convey the essence through genus and differentia. His definitions were not at all concise or rigorous. For example, consider these "characteristics" of "mucous fever": There is general debility and irritation, especially of the mucous membranes of the intestinal tract. This disease is determined by a delicate constitution, poor nutrition, a sedentary life, a marshy dwelling-place, excessive study, unhappy spirits. From these causes can result fevers, continued, remittent, or intermittent, endemic or epidemic. The course is generally slow, and symptoms are not intense. The pulse is full and slow. There is shivering without tremors, moderate fever, abundant secretion of viscid mucus in the alimentary tract, frequent vomiting, diarrhea, somnolence, pains of the limbs, acid odor of the sweat, sometimes prolonged salivation.[27]

This lengthy, non-specific description is a very far cry from the logical apparatus, the precise definitions, and the exact differentia that the botanists and the early nosologists attempted.

Pinel, of course, had a closer grip on reality, for he realized that artificial, rigid distinctions only impeded scientific progress. Successive editions of his book drifted further and further away from his predecessors. By the fifth edition, in 1813, he had shifted nomenclature and terminology, and the entire nosologic apparatus receded markedly in importance. Definitions and the arrangement into genera and species were relegated to the back of the book. Thereafter transition to a completely modern textbook format was not difficult.

But nosology continued important, even though classes, orders, genera, and species became outmoded terms. Benjamin Rush clearly showed that all attempts to deride classification are self-defeating. Classification is a practical necessity. As Caesalpinus pointed out, unless we can identify the like and the unlike there is no science. Nosology tries, in the field of disease, to systematize the various identifications and render them orderly. The results must be practically useful and intellectually satisfying. If in either respect one particular arrangement falters, then it is not a good arrangement. But we do not, therefore, dispense with all arrangement. We simply try to find a better one, that is, a new, improved nosology. This the medical writers started doing in the eighteenth century, and the process, now somewhat disguised, has continued right up to the present moment. The eighteenth-century authors loaded themselves down with cumbersome nomenclature and an inadequate philosophy of science and hence fell into obscurity, becoming almost a little disreputable. But their most violent critics, now as well as then, perform exactly the same functions.

MEDICINA. VIII

The Development of Medical Ethics

The past decade has seen renewed interest in those subtle relationships between doctor and patient and between doctor and doctor that we call medical ethics. The question, What constitutes upright conduct? again stimulates critical examination from both laymen and professional men. Existing formulations of ethical behavior are being reanalyzed and new codes drawn up. All this renewed activity, reflecting as it does contemporary twentieth-century problems, has its background in the eighteenth century, when medical ethics in the modern sense first became crystallized.

The concept of "ethics" has had a twofold implication. There is a metaphysical aspect, which concerns the nature of the Good as an abstract and universal property: as philosophers we connect ultimate realities and the ultimate verities, and from them derive our values. We dwell more or less in an ivory tower, wherein time has no urgency, and, instead of ugly concrete situations, we find only beautiful smooth abstract universals. On the other hand, we may face various immediate, concrete, specific events—real, urgent, particular situations demanding a prompt answer and of such a nature that indecision, delay, or avoidance are themselves a form of answer. William James called these "forced options": something MUST be done. If this type of situation engages out attention, our ethical studies take a different form, earthy and practical. Our codes are then rules of guidance to meet specific problems. Our regulations offer practical means of achieving limited and particular objectives rather than furnish insight into the Good *qua* philosophic principle.

Medical ethics, considered as a practical system of regulating conduct, has had a very long history with which, however, we will have little concern. Our present-day ethical codes arose not from Hippocrates or Hammurabi, or from the fragmentary medieval regulations that have survived, but directly from Thomas Percival, who wrote at the end of the eighteenth century. Percival tried to resolve the wrangling and discord which had punctuated the entire century and which, in turn, had developed out of a definite socio-economic background. Medical practitioners faced certain concrete difficulties relating to their professional brethren and the lay public. In this chapter we can merely outline a few of the problems, and some of the solutions, which led to Percival's remarkable text and, by extension, to our present medical ethics. The problems, be it said parenthetically, have not changed as much in the past 250 years as we might like to think.

One stumbling block to realistic medical ethics has been the belief that, somehow, physicians differ from other men. Today, various programs of mass entertainment have glorified the physician as a being almost superhuman. There is a faint historical support for this viewpoint, since in primitive times the medicine man and the priest were either the same individual or were closely allied. They stood apart from the vulgar crowd as possessing very special virtues not shared by the population at large. This belief is, of course, grave error and quite fatal to any genuine understanding of medical ethics. Not a few articulate physicians have protested against this view. Writing in 1799, William Wells declared that physicians "cannot by reason of the discipline of their profession, claim exemption from the moral infirmities, to which the other inhabitants of this country are subject."[1] And that center of controversy James Gregory wrote satirically, "Whatever the majority of us may be, I am afraid we are not all perfect angels. Some of us at least appear to be made of the same flesh and blood and to be subject to the same frailties, and passions and vices, as other men."[2] Only the highly imaginative, with both feet already in the clouds, could suppose otherwise.

These propositions indicate the twofold ethical perplexities which have faced and still face practitioners. Simply as men, as members of the human race, they encounter problems they must meet, as do other men, and thereby secure social approbation or, sometimes, condemnation. But then, as members of a specialized and highly trained group, they encounter additional specialized problems not shared by others. Social, professional, and economic groups, if closely organized, will have their own codes of behavior, their own standards, their own demands of loyalty, adapted to the special interests and the special problems of the group. We may say that any professional group confronts a general ethic and a special ethic, whose demands are by no means the same.

Medical ethics, in its modern development, is connected inseparably with guild organization. Special groups, whether professions or trades, have evolved their own codes or regulations, binding on their members although obviously not binding on anyone who is not a member. Such regulations, whose importance is proportional to the power and influence of the sponsoring group, became historically relevant to medicine during the early sixteenth century, when the Royal College of Physicians was founded. This organization was in essence a feudal guild, or at least a direct lineal descendant thereof, an example of that system which gave a protected favored position to its members in return for specified services.

We usually think of feudalism as a political principle, wherein one party, the suzerain, exacted services from a vassal but in return promised to protect the vassal from his enemies. It was a fairly explicit contractual relationship. The same relationship, however, obtained in the economic sphere. The king, or ruling power, could contract with a group of artisans, tradesmen, or other workers, granting them certain privileges in return for certain performances. The motive influencing the workers was ordinarily a desire for social or economic betterment or relief of contemporary economic distress. For example, in certain trades or professions unrestrained competition might depress all standards. Poorly qualified workers might flood the market with shoddy goods or incompetent services. The conscientious and well-qualified workers would suffer and so would the general public. The remedy was the guild. An organized group received from the ruling authority a charter granting a strict monopoly in certain goods or services. In return the guild guaranteed a high standard of quality. Society as a whole benefited from the improved standards. The guild members benefited from the monopoly. The only ones who did not benefit were any competitors whom the monopoly excluded. But this exclusion was precisely the consummation devoutly wished.

The guild, under charter, could prescribe its own rules within a specified jurisdiction, and the whole power of the state would (in theory) enforce these rules against all interlopers. This was a familiar pattern in the Middle Ages and has persisted with modifications to the present day.

The Royal College of Physicians had, indeed, tremendous monopolistic powers. The haze of time now blurs the jagged social problems which were ethically significant in the sixteenth-century. It would be a little naïve to attribute to the college any strong motives of public welfare. To be sure, guilds have traditionally established standards of performance, guaranteeing a certain level of competence and holding their members accountable for meeting those standards, but in return they have exploited monopoly for their own betterment. Whether in the dealings of the Royal College proper balance was struck or whether public welfare became subordinate to private greed is a problem for ethical evaluation.

Long before the economic interpretation of history was fashionable, critics impugned the motives of the college founders. Dr. Wells, as a result of personal experience, bitterly maintained of the founders that, since the "great bulk of mankind" was "unable to judge of the truth . . . or the proprieties of their practices," a number of physicians persuaded the sovereign that they were "the only fit persons to take care of the health of his subjects; while in truth the great object of their combination is to establish a monopoly of medical employment in their own favor. This I believe to be the real origin of our College of Physicians, notwithstanding the praises which have been lavished upon its founders."[3] And Wells rightly pointed out that the charter was granted to the College of Physicians in the age of monopolies. Modern idiom would say that large prizes were "up for grabs" in the early sixteenth century. Great opportunities were opening up, and the physicians wished to have their share, or perhaps slightly more.

Wells's stricture may not have been entirely valid, but there is ample evidence that the college policies deserved criticism. We have already discussed the conflict between the College of Physicians and the apothecaries at the end of the seventeenth century. There was no doubt at all that apothecaries committed abuses against the public welfare or that they infringed upon the charter rights granted to the College of Physicians. But there is also no doubt that the licensed physicians failed to meet public needs. Authority and responsibility are correlative. Whoever has responsibility must, to fulfil the demands upon him, be granted adequate authority. But the converse proposition is neglected all too often: when a limited group possesses authority, that group has a responsibility. The guild, like its modern deviatives, existed only on a *quid pro quo* basis. To grasp the *quid* without rendering suitable return, to flout public welfare for private self-aggrandizement, creates an unstable situation that is basically immoral.

The conflict between physicians and apothecaries was fundamentally economic. Medical ethics as we know it today has one major root in the eighteenth-century problems of competition. At that time the physicians formed a very restricted group, but the apothecaries did not. The physicians increased very slowly, the apothecaries very rapidly. At the beginning of the seventeenth century there were only twenty physicians in London and one hundred and fourteen apothecaries. At the beginning of the eighteenth there were no more than eighty physicians, out of the entire college roster, who practiced in London, but there were an estimated one thousand apothecaries.[4] There was no need for this number, at least not for this number of drug dispensers. A contemporary estimate declared there were ten apothecaries for every physician. Of these ten, one might be "covenanted" to a physician, that is, have a secret agreement to split fees or, in eighteenth-century terms, "a private Compact of going Snips out of the most unreasonable rates." Another might earn an honest living by "wholesome Trade of fitting our Chirurgeons Chests for the Sea, and supplying Country Apothecaries" but the other eight were "compell'd either to sit still, or to quack for a Livelihood."[5] The sick must support all of these. And since apothecaries wished to live "handsomely," they charged high prices for their medicines. They also enlarged their scope and not only sold drugs but prescribed them as well.

The few physicians had an ethical problem of protecting the public from untrained empirics. The charter of the College of Physicians imposed the duty of safeguarding the public from the incompetent. At the same time the physicians wished to protect themselves from excessive competition. If an altruistic duty could be made to coincide with heightened self-interest, there would be a double satisfaction: a job well done, and self well cared for. The College of Physicians tried to do both at once—and succeeded in doing neither. It did not protect the public, that is, it did not raise medical standards; and, instead of soundly improving the physicians' own status, it undermined their position. Over the centuries the college undertook numerous prosecutions to carry out its dual goal, but its motives became highly suspect. Of the earlier of these prosecutions it was justly remarked that "unauthorized practitioners were deemed worthy of punishment or otherwise, not as they killed or cured his Majesty's subjects, but as they were insolent or humble to the College." The same author declared that the behavior of the college "presents a melancholy record of ignoble compromises, or inglorious defeats, and of victories, if possible, still more inglorious."[6]

As we have already seen, the college suffered a great and merited defeat in 1704, when the House of Lords freed the apothecaries from the immediate domination of the physicians. Although the college still had power to harry the apothecaries—for example, by inspections and condemnations of drugs—in general the conflict shifted to another front.

The changes in medical education have already been mentioned. With the growth of the Scottish universities, the development of hospital teaching, and the rise of private academies, excellent practical and theoretical instruction, keeping pace with advancing medical science, became readily available. But standards were nevertheless lamentably lax. Scottish universities throughout most of the eighteenth century might sell their diplomas without adequately examining the candidates. Hence, although many Scottish graduates were excellently trained, the mere degree itself did not necessarily imply sound education or training. Obviously, such a setup produced many doctors of medicine who were poorly qualified.

The Royal College of Physicians had the duty to maintain high standards and to guard the dignity and good name of the profession. Membership in the college was supposed to represent the highest level of professional attainment. Quite properly, therefore, they refused to recognize a Scottish degree at face value. It was not that they excluded men with Scottish or foreign degrees. Instead there were two grades of membership: the fellows, almost exclusively graduates of Oxford or Cambridge, and licentiates, who were graduates of other universities. Holders of Scottish or Continental degrees were theoretically eligible to become licentiates if they passed the college examinations, which required considerable classical learning. In the middle of the eighteenth century there were fifty-six fellows and only twenty-four licentiates. By 1783 the fellows numbered only forty, and the licentiates seventy-three. Yet the fellows, who alone could vote, had exclusive control over all policies. There was a sharp class distinction, a group that was *in* and a group that was *out*. This, of itself, is not necessarily bad, if the *ins* are adaptive to circumstances. But if control lies with a reactionary, self-perpetuating old guard which cloaks selfish moves with pious remarks about the public welfare, then there is no adaptation. Even though the Scottish universities greatly improved their standards, and even though their graduates became the most eminent practitioners in England, the college held fast to its restrictions.

The licentiates became very restive. Since they saw no reason why they should be denied fellowship, there was vigorous revolt; licentiates sought full privileges, including the vote, in accord with their merits. The old guard bitterly resisted all changes of status, refusing to grant fellowship even to such outstanding physicians as John Fothergill and William Hunter. The matter came to violence when a party of licentiates forcibly broke into the locked council chamber of the fellowship. Test cases were brought to court to compel the college to admit to fellowship properly qualified licentiates. Since the law was quite clear, however, the courts upheld the reactionary bylaws governing elections. Nevertheless, Lord Mansfield pointed out from the bench that the college policies, although not illegal, were too narrow and should be changed.[7] The college did relent a little, but only nominally.

The hypocrisy of the college, its refusal to make genuine reforms, increased toward the end of the century. The case of William Wells led to considerable publicity and the well-known "Letter to Lord Kenyon." Wells, an eminent scientist who discovered the laws governing formation of dew, a member of the Royal Society, and a prominent physician, had repeatedly but vainly tried to advance from licentiate to fellow of the college. As the result of Lord Mansfield's criticisms, the college had theoretically set up a mechanism by which this might take place. But Wells, although sponsored by the eminent doctors David Pitcairn and Matthew Baillie, met only evasion and parliamentry trickery and was finally rejected without even being admitted to the examination. There is little point in repeating Wells's indictment of the bad faith that dictated and administered the college bylaws. The change in status from licentiate to fellow was possible, but in actuality it almost never occurred. Wells pointed out that the struggle was essentially between the graduates of Cambridge and Oxford and the graduates of Scottish universities. The British graduates, constituting the college fellows, were not only party to the struggle, but they were also the judges who made the rules in the struggle. When one side is both contestant and judge, a rare moral rectitude is necessary to preserve even the semblance of justice. The difference between the examinations for fellowship given an Oxford graduate and the examinations to which a Scottish licentiate was subjected is a case in point.[8] The fellows in the College of Physicians did not have the necessary moral rectitude. To be sure, Wells was eventually offered fellowship status, but by then he indignantly refused the offer.

Not until the nineteenth century did some reforms overtake the college and the narrow partisan spirit change for the better. The actual change therefore lies beyond the scope of our period. During the entire eighteenth century the College of Physicians represented moral and ethical failure; through selfish exploitation of monopoly it revealed the defective morality of the period and of the medical profession.

Within the medical profession enmities were rife. Conflicts between practitioners were rancorous, and basic jealousies swayed behavior. There are some well-documented examples of discord that are very illuminating and illustrate the temper of the period.

William Withering, who discovered the diuretic power of foxglove in cardiac edema, was involved in such a dispute. Born in 1741, he served an apprenticeship as an apothecary and then matriculated at the University of Edinburgh, receiving his M.D. four years later, in 1768. He was a scientist, a renowned botanist, a man of letters, and a very successful physician, who earned an annual income of 2,000 guineas by the time he was forty. He had settled in Birmingham, enjoyed a very extensive practice, and was one of Britain's leading physicians.

One night in 1788 Withering received a letter from an apothecary, urgently requesting his immediate attendance on a woman twenty-six miles distant. Although he had had a long, tiring day, Withering traveled that night to see the patient, a Mrs. Houlston, in consultation with the apothecary. The patient, it seems, had previously been seen by Dr. Robert Darwin, son of the famous Erasmus Darwin and father of the more famous Charles Darwin. Withering, believing he was to take control of the case, neither met nor consulted with Dr. Darwin. Indeed, he disagreed with Darwin's diagnosis, canceled the latter's medication, and himself prescribed quite differently. When, after an interval, Dr. Darwin had had occasion to see the patient, he loosed the vials of his wrath on Dr. Withering. He wrote, "Your ungenteel behavior to me in the case of Mrs. Houlston in not acquainting me that you was there, when you knew I was in the town and had been previously concerned with her, was not very consistent with the character of a gentleman but the activity with which you have slandered me on this occasion is a matter of much more serious consequence."[9] Here followed a medical discussion whether the patient did or did not have a fever or an inflammation of the liver. The letter continued, "Sir, if I hear that you again slander me—and I am very likely to hear it if you do,—I shall treat you in a very different manner, not agreeable to your dignity. . . . every liberal person sees through the paltry motives which have always induced you to slander those of your own professions, among the other mean arts by which you attempt to support your business."

Darwin was a youth of twenty-two who had received his M.D. degree only the year before and had been in practice but a single year. Withering nevertheless answered in detail to refute the charges. He went on: "The idea I had taken up of the case was so different from yours, that I had reason to believe the adjustment of such a difference would have taken up a good deal of time, and I had no such time to spare, either for the purpose of idle ceremony or useless altercation." He discussed the medical aspects, but his remarks were not all sweetness and modesty. "That I have *slandered* you . . . is untrue, but the difference in our opinion . . . was too great . . . and as I could not protect you in any way in which I ever wish to cover the errors of a young physician, I could not, in justice to myself and my patients, act otherwise than I have done. . . . The remainder of your letter breathes such an air of insult, and such a spirit of impertinence that I do not think proper to answer it."

In return Darwin defended his own clinical acumen with further detailed medical analysis, interspersed with studied insults. For example, he closed one paragraph by saying, "I leave you in possession of these facts without further comment, they ought to humble you; but your pride dazzles your eyes, and will not permit you to see your own ignorance." And, after much more in the same vein, he closed by saying, "You are one of those characters of whom the enmity is far less dangerous than the friendship." Withering, in a brief note, replied, "Possessed, therefore, as I am of self-satisfaction, of the good opinion of the world at large, and of medical men in particular, your enmity or your friendship, your good or your bad opinion, are to me equally insignificant."

There was considerably more correspondence, involving the patient's husband as well as the antagonists. To make the stench worse, the first three letters, two by Darwin and one by Withering, were published as a pamphlet and distributed to physicians and laymen in the community. Darwin denied being party to this step; Withering obviously was not; suspicion falls on Darwin's father, Erasmus Darwin, who treasured a deep hatred of Withering and unquestionably abetted his son.

We are not concerned with the merits of the case, whether Mrs. Houlston had hepatitis, whether rhubarb was properly indicated, or whether she should have received calomel or bark. Significant, rather, is the abuse which one reputable physician would lavish on a colleague, attempting by the widest possible publicity to destroy his reputation.

It may not be true that the eighteenth-century physicians spent a disproportionate amount of time and energy in quarrels, but it seems so to our modern taste. Perhaps it was merely that they had fewer inhibitions. They often chose to let themselves go and, moreover, to express themselves in pamphlets whose circulation the principals, as well as the booksellers, sought to promote. Today a physician may circulate reprints of scientific publications, hoping thereby to secure favorable attention. In the eighteenth century, instead of reprints, physicians circulated pamphlets or articles which sometimes were purely scientific, sometimes controversial but still concerned with science, and sometimes only abusive and scurrilous attacks on someone else. One way to attract attention was to engage in controversy, preferably with someone well known. If he could be stung to reply, there would be further opportunity for more writing.

Pamphlets and articles were eagerly read. Even though the references were veiled and pseudonyms were substituted for real names, the persons involved were usually quite readily identified. The medical writings of the latter eighteenth century were not quite so bad as the political pamphleteering of the era, but there was often a definite similarity. One of the more famous attacks fell on John Coakley Lettsom, who was lampooned under the name of "Dr. Wriggle" in a brilliant article that is still amusing. Dr. Wriggle practiced the "Art of Rising in Medicine," that is, the eighteenth-century version of "how to get ahead in the world." There were numerous maxims furnished to accomplish this, numerous ways to bring your name before the public: by securing subtle newspaper notices; by assuming the appearance of learning and gravity; by engaging in controversy with public figures; by pretending always to be very busy; by forcing yourself into the company of great men; and so on. The squib gave pointed illustrations, witty and entertaining and probably not without considerable bitter truth. The article does not lend itself to quotation but is given at length in the readily available biograpry of Lettsom.[10]

A very serious medical controversy of great ethical import involved John Fothergill, who felt moved to uphold by himself the dignity of the medical profession. As already mentioned, the mere degree, doctor of medicine, did not necessarily indicate adequate medical education. Degrees might be purchased as well as earned, or they might be acquired after very scanty training. In 1768 a Dr. Samuel Leeds, holding a degree from Edinburgh, obtained an appointment as physician to the London Hospital. Leeds was a Quaker, as was Fothergill. The latter, a well-educated and outstanding graduate of Edinburgh, resented the newcomer's appointment and was "emphatic" in expressing his disapproval. He cautioned a colleague to take care that Dr. Leeds "do no harm to his patients." The admonition was effective. Following complaints of incompetence, the governors resolved that no one could hold the post of physician who was not a licentiate of the College of Physicians. Leeds, forced to take an examination, failed and had to resign his position in the hospital. For all this he blamed Fothergill and made a formal complaint to the Society of Friends through the local group, the Monthly Meeting. Since Fothergill was a prominent member of this group, the affair rather dragged. Leeds then appealed to the higher assembly, the Quarterly Meeting. A committee met; both doctors agreed to submit to arbitration and posted bond to abide by the result. The charges were that Fothergill had accused Leeds of obtaining his degre surreptitiously, had warned a physician that Leeds might do harm to his patients, and had said that Leeds and another brought disgrace to the society. The committee, all laymen, found in favor of Leeds, and assessed Fothergill five hundred pounds damages.[11] It is clear that the decision was not based on expert medical testimony but reflected, rather, the Quakers' dissatisfaction with Fothergill's dictatorial manner. There was the feeling that he was injuring the weak and was harsh and unfair in his treatment of a rival.[12]

Fothergill refused to abide by the decision. In a letter to William Cullen he declared that his friends "insist upon my endeavoring to vindicate myself from the stain of defamation. . . . I have yielded to their importunities; and conscious of my innocence, both in act and intention, I am in hopes of giving ample proofs of it."[13] And later he commented in another letter, "And it is not more to be wondered at that he [Leeds] prepossessed three weak, though honest, men in his favor so strongly as to render them deaf to all reason or force of evidence, than that he should have had art enough to obtain a degree."

Later in the same letter, he complained, "It hurts me not a little that the affair has already been so widely published. . . . The gross ignorance of Leeds, the injustice done me on his account, the partial decision in his favour, all contributed to interest a multitude of persons in the event. The University will be the greatest sufferer."[14]

At any rate, Fothergill refused to abide by the decision. He felt it "to be a public duty to secure a more equitable judgment, and so clear the society from dishonour."[15] He expected the matter to go to the ultimate church authority, the Yearly Meeting. Leeds, however, appealed to the courts to compel observance of the bond. The matter was agitated for two years among the Friends, who hoped the case would not come up in the public courts, but Leeds was firm, and the case finally came before the King's Bench. Here, however, the court, after hearing Leeds' side, set aside the award without ever calling Fothergill's counsel.

It was clear that Leeds was ignorant. The judge held that Fothergill had only done his duty in acting as he had. Leeds died in poverty the following year. During his last illness someone anonymously supplied all his needs. It is believed that the unknown benefactor was Fothergill himself.

This case suggests many interesting points of character and casts much oblique light on contemporary medical practice. It also points up the lack of any central authority to hear complaints and keep the medical profession in order. Within very broad conventional limits physicians acted pretty much as they pleased. Behavior was not adequately covered by statute, nor were there local regulatory codes which today we call codes of ethics. Vague ethical generalities, concepts of the Right and the Good, overtones of the Sermon on the Mount and the Golden Rule, notions of what properly befits a gentleman, these were all current; yet obviously they did not produce smooth professional relationships.

There are, perhaps, two major ways of improvement. One is educational. Let the recognized leaders of the group hold up certain ideals of conduct, ideals which make an immediate appeal to the "moral sense" of youth. Let teachers inculcate these ideals, illustrating them by example as well as by precept. Let all members of the group strive earnestly to keep to these ideals. In such a fashion, perhaps, a strong morale will be built up, and a "moral sense," strongly developed, will ennoble the entire profession.

The second way is a little more earthy. Let the group not only hold out ideals of conduct but also codify certain specific rules of acceptable behavior; let there be means of enforcement, so that the rules are not only a pious hope but a concrete reality, carrying a penalty for infraction. The group, if strong, can enforce certain standards of behavior. Then the explicit rules may be called a code of ethics.

The two ways are not mutually exclusive but represent two extremes. The one is ultimately reducible to pure voluntary co-operation, the other to pure coercion. In practice a combination of the two is desirable. There should be an appeal to the "moral sense" through education, but there should also be some degree of formal codification and penalties for infraction. The later eighteenth century saw considerable progress along both these lines. At least the problems were recognized and first steps taken toward a solution.

Exhortation to high ideals was furnished by John Gregory, a member of the brilliant Edinburgh faculty. Gregory, educated in Aberdeen and Leiden, had practiced in London but came to Edinburgh in 1765 at the age of forty. He quickly established a successful practice and in 1766 was appointed "Professor of the Practice of Physic," ahead of William Cullen, who also had sought that chair. Gregory lived only until 1773, but he published a very influential volume, *Lectures on the Duties and Qualifications of a Physician,*[16] which eventually helped lay a foundation for a formal code of medical ethics.

The book was a series of lectures to medical students. Of the six chapters the first two dealt more or less with the duties of a physician and the qualities desirable in a medical practitioner. The presentation is brilliant, entirely applicable today as well as two centuries ago, and breathing a kindliness and fervor that we associate with deservedly outstanding physicians. Gregory mentioned two alternatives, that medicine may be considered either an art or a trade. He was glad to address young students, for "youth indeed is the season when every sentiment of liberty, of generosity, and of candour, most easily find their way to the heart," while age, "which in some respects improves the understanding, in others . . . checks the ardent pursuit of science and truth, and shuts the heart against every manly, enlarged and generous sentiment."

Gregory tried to describe systematically the qualities which go to make up a physician and the educational course needed to fit a young man for that profession. Among intellectual qualities he stressed various aspects of judgment accompanied by "command of the temper and passions, . . . presence of mind, composure, steadiness, and an appearance of resolution." More significant for our purpose are the moral qualities. Gregory emphasized the "humanity" of a physician, "that sensibility of heart which makes us feel for the distresses of our fellow creatures," a basic sympathy through which the patient considers the physician a friend. Such sympathy, he said, is often affected and hypocritical, but Gregory referred to a genuine sympathy which will be accompanied by good humor, good nature, and patience. He commented slyly on the changes that a physician's manner sometimes undergoes. When starting practice, the doctor is "affable, polite, humane, and assiduously attentive," but after achieving financial success he becomes "haughty, rapacious, careless, and often somewhat brutal."

After discussing the moral virtues generally—discretion, sobriety, temperance, and others—Gregory considered the "decorums" peculiarly incumbent on a physician. "Decorum, decency, and propriety" are notions founded partly in "nature and common sense" and partly in "caprice, fashion, and the customs of particular nations." In this chapter Gregory examined certain concrete problems of behavior. He mentioned, for example, the classical problem, How much should the doctor tell the patient in a dangerous illness? Should he deviate from the truth? As do present-day medical moralists, Gregory hedged: "It may be proper, in the most prudent and gentle manner, to give a hint to the patient of his real danger." But, of course, since any individual prognosis may be extremely fallible, perhaps the patient will recover if he does not know how very sick he is, if he does not know that the physician despairs of recovery. In any event the doctor, even if he does not tell the patient, must certainly tell the relatives.

Gregory inserted a fine compassionate touch by emphasizing the physician's duty to continue his visits even in hopeless cases. "It is as much the business of a physician to alleviate pain, and to smooth the avenues of death, when unavoidable, as to cure diseases." His very presence will be helpful, even if his skill is unavailing.

The physician must not let professional quarrels and jealousies interfere with the patient's welfare. Those called in consultation must lay aside all resentments and think only of the patient. Consultations are highly desirable, provided they are carried out in good faith, with honor and due secrecy. A physician should refuse consultation if he feels he cannot act with good temper and sound judgment. But a consultant should not refuse to attend merely because he does not approve of the requesting doctor's medical training or the university from which he got his degree. Medicine, in brief, is "a liberal profession, whose object is the life and health of the human species, a profession to be exercised by gentlemen of honour and ingenuous manners; the dignity of which can never be supported by means . . . that only tend to increase the pride and fill the pockets of a few individuals."

We must stress, parenthetically, the great importance of consultations in eighteenth-century medical practice. Perhaps because assured knowledge was so limited did mere opinion count for so much. Consultations furnished a means of sharing opinions. Gregory, as a Scot, must have felt keenly the discrimination against Scottish physicians that obtained in London. The College of Physicians looked down on Scottish medical graduates and often expressed their disdain by refusing to consult with such an inferior order. An amusing yet bitter commentary on these practices we find in a contemporary letter from one William Cumming, a Scottish graduate, who wrote that the British graduates "looked on the Scott doctors as of a class inferior to themselves, and sometimes refused to consult with them." In one of his own cases in which an Oxford graduate was asked in consultation, "The insolent Oxonian declared, *totidem verbis,* that he had no objection to my person, parts, or skill in my profession, but that he thought it a duty which he owed to that University of which he was a member, not to consult with any one who had not been educated at an English University. The consequence in this particular case was, that I was employed in the gentleman's family for fifteen years after (so long as he or any of his family lived,) and that the Oxonian was instantly dismissed."[17] The outcome of such prejudice, however, was not always so favorable, and Gregory may well have had some such experiences in mind when he wrote about consultations.

In his lectures Gregory mentioned the difficult relationships between physician, surgeon, and apothecary. After reviewing the historical development, he condemned that separation of physicians from surgeons which produced divergent interests. Purely technical surgery did not require medical training; nevertheless, since surgeons often acted as "physicians in ordinary" (that is, as family doctors, or general practitioners), it happened that "in many places physic is practiced by low illiterate men, who are a disgrace to the profession." Such was the evil resulting from separating medicine from surgery, and the same applied to the medical practice of untrained apothecaries.

Gregory bitterly lamented current situations wherein all too often the physician was dependent on the apothecary.

But on the other hand he cautioned against pride in mere education alone (in a sense, cautioning against the Oxonian error mentioned above). He said, "A doctor's degree can never confer sense, the title alone can never command regard. . . . If a surgeon or apothecary has had an education, and acquired the knowledge of a physician, he is a physician to all intents and purposes . . . and ought to be respected and treated accordingly." The "candid" or "liberal" physician will not take advantage of nominal distinctions. He "will feel no superiority, but what arises from superior learning, superior abilities, and more liberal manners. He will despise those distinctions founded in vanity, self-interest, or caprice; and will be careful, that the interests of science and mankind shall never be hurt by a punctilious adherence to formalities."

Other "decorums" also received attention. How should the physician dress? Should he show any distinguishing formality of manners? Should he avoid idiosyncrasies of behavior? Gregory discussed these briefly. He stressed, as well, that it is not below the physician's dignity, when necessity calls, to meet the disagreeable demands which illness may entail, even for menial nursing. Performing various offices to relieve the patient is a duty to humanity. But it is degrading and unbecoming a gentleman if he "encroaches on another's province" when there is no necessity (that is, when others are available who would be paid for such services).

And so with various other problems. How often to visit a patient? Is it desirable to use secret remedies, or "nostrums"? Should the doctor acquaint the patient with the nature of the remedies? There are no dogmatic answers, but Gregory suggests his own viewpoint, which even today seems sound.

The remainder of the book concerns not ethical problems but educational features and some general principles of the scientific method. We cannot tell just how influential were the ethical portions. Presumably there was little immediate effect, yet undoubtedly Gregory's *Lectures* helped to create a climate of opinion, to establish a tradition that eventually modified the older crude materialism.

It is very refreshing to read Gregory, for here breathes the spirit of a truly noble man holding out to young and impressionable students certain maxims and ideals of conduct which we all applaud, even if we do not always conform. We feel better for having the ideal explicitly pointed out, even if we do not always follow it.

Clearly, any specific exhortations imply that contemporary conduct falls short of the goals set. We have already seen some of the defects of medical practice based on jealousy, pride, economic rivalry, or lust for power. Gregory's lectures are not abstract philosophical analyses but straightforward talks on life's ideals, as seen by a leading and respected physician who would like to steer the young and impressionable into the right path. In essence he says, "This is the Right as I see it."

Quite ironically, Gregory's own son James was the most flagrant offender against good taste, decent behavior, and all those gentlemenly canons which might improve medical relationships. The son was precocious and brilliant. He was still a student when his father died, leaving vacant the professorship of the practice of physic. Cullen succeeded to this chair, leaving open the professorship of the institutes of medicine. The Edinburgh authorities kept this office vacant for four years until James Gregory had gained his degree and profited by two years of foreign study. The young man, then only twenty-three, became professor, succeeding the great Cullen. James Gregory was a good teacher, had an extensive practice, and supposedly was a favorite with the students. Although he published a textbook on physiology, most of his writings were polemical and controversial, extending to well over two thousand printed pages.

As one admirer described him, "He was a curious and excellent man, a great physician, a great lecturer, a great Latin scholar and a great talker. . . . the popularity due to these qualities was increased by the professional controversies and the diverting publications by which he used to maintain and enliven them. The controversies were rather too numerous, but they were never for any selfish end, and he was never entirely wrong. Still, a disposition towards personal attack was his besetting sin."[18]

This is perhaps a triumph of understatement. Possibly the most disgraceful episode in all British medical history was Gregory's vicious attack on John Bell, comparing him to a mad dog and finally securing his exclusion from the hospital. Bell, in 1799, gave up all teaching and contented himself with being merely the leading practicing surgeon in Edinburgh. Comrie discussed this case briefly, but I have not consulted the original pamphlets.

If we venture just over the line into the nineteenth century, we can study at first hand some of Gregory's controversies. There was, for example, his attack on the Edinburgh Royal College of Surgeons and the surgical attendants at the Royal Infirmary. When the infirmary was established in 1738, the College of Surgeons made formal arrangements whereby their fellows would attend in rotation, giving their services to the indigent. Gregory complained that this was a vicious practice; he implied that the junior fellows were inadequate, not sufficiently trained, and were merely practicing on the unfortunate poor. He attacked the alleged contract whereby all the college surgeons maintained the right to practice on the poor. With great care he insulted all the surgeons in Edinburgh, offering, for example, such gratuitous remarks as, "I am sure I would not trust one paw of my great Newfoundland dog to a consultation of thirty or of three hundred of them."[19] His writings throw considerable incidental light on medical practice of the times, even though we cannot accept Gregory himself as a reliable witness.

His attack drew forth a retort, the *Answer for the Junior Members of the Royal College of Surgeons of Edinburgh to the Memorial of Dr. James Gregory,* written, it was later disclosed, by John Bell.[20] Bell failed to match the invective and satire of Gregory, but he did present a very adequate defense and gave a good exposition of the state of surgery at that time.

Gregory replied with a second quarto volume, the *Additional Memorial,* which is a dreary reply to a reply.[21] Bell kept silence for some years, until he published in 1810 the well-known *Letters* on the education of a surgeon, addressed to Gregory.[22] This volume, containing much interesting contemporary material, nevertheless savors too much of a reply to a reply to a reply, a "he said" and "I said" and "he said." It suggests an author who had been lamed and warped; whose entire personality was distorted by Gregory's vicious pen.

With great impartiality Gregory also attacked the Royal College of Physicians in Edinburgh. For the preceding half-century the college had faced serious problems regarding its membership. By its charter any M.D. from any Scottish University could automatically become a licentiate. Some university standards were not very high, as we have seen, and there was no formal way of distinguishing a bona fide physician from an apothecary who obtained a surreptitious degree. Starting in 1750 the college passed a series of bylaws regulating the membership. One bylaw in 1754 declared that no licentiate or fellow residing in Edinburgh could keep an apothecary shop or practice pharmacy. Other regulations excluded certain surgical practitioners and midwives.[23] There was much conflict over the restrictions, some of which were rescinded, others not enforced. A strong movement arose to repeal the 1754 prohibition and to allow physicians to furnish medicine for their own patients. The question was agitated in 1796 and again in 1804, when a committee recommended that the 1754 restriction should apply only to those who kept public apothecary shops selling at retail and not to physicians prescribing for their own patients. Because of opposition to this recommendation the committee decided to reconsider and withdraw part of its report.

Gregory, who was prominent in the opposition, wrote a pamphlet entitled *Censorian Letter,* a long-winded, scurrilous effusion opposing any change in the 1754 act and heaping abuse on those who favored change.[24] The tone is set in the Table of Contents, where the very first section, twenty-five pages in length, bears the title "Preliminary observations on the impropriety of agitating again, in our College, a Proposal, the same in substance with one which, but a few years ago, had been reprobated with indignation, as not only inexpedient, but base and dishonorable; on the peculiar impropriety of the manner in which this is attempted at present; by Chicane, Deceit, and a Falsification of one of our laws; and on the necessary consequences of such conduct in any of our members." This, be it emphasized, is only a title. The whole text, wordy and windy, is labored and sophomoric, sometimes exhibiting a feeble wit. It dispenses abuse by indirection, metaphor, and innuendo rather than by direct name-calling. Even the extensive classical quotations and pseudo-logical rigors do not raise it above a bratty, smart-alecky level.

The Royal College of Physicians struck back, and hard. Gregory was formally censured in 1806 for his accusations, which "are utterly groundless, and unwarranted and . . . very disrespectful, if not a direct insult to the College." The college expressed "with the deepest regret, yet in the strongest terms of disapprobations, their sense of the indecorous and improper conduct of Dr. Gregory," which merited their "very severe censure."[25] He tried to enter a defense but was censured again in 1808 for direct and proven falsehood.[26] In 1809 the college published the *Narrative,* exposing Gregory's falsehoods and perverted facts. This refutation is incomparably more effective and convincing than Gregory's own books.

In retrospect, judging by Gregory's writings, we might in modern terms consider him a psychopathic personality with paranoid trends. A little pathetic, in view of his personality, are his remarks on the progress of medicine: "The great Volume of Nature lies open to physicians of every sect, and every opinion; but they must learn to read it fairly, like men of science, who seek for truth alone; not perversely, like disputants contending for victory."[27] No one could be more polemical and quarrelsome than Gregory. His words illustrate perfectly the dictum: "Don't do what I do, but do what I say." His life stands as a challenge to organized medicine to keep its own house in order, a challenge which the eighteenth century did not meet. His life also illustrates how futile is mere exhortation. His father's precepts, admirable as they were, did not have much effect on James Gregory.

More significant than either of the Gregorys was Thomas Percival, who was indeed a rare figure. Medicine for him was a familial calling. Although his own father was in business, his grandfather and uncle were physicians, and he himself passed the tradition on to his sons. Thomas

Percival, born in 1740, had a sound education at Edinburgh and in London but obtained his medical degree at Leiden in 1765 during a period when differences between students and faculty at Edinburgh provoked a student migration to Leiden. After acquiring his degree, he settled in Manchester, where he became a leader not only in medicine but in the cultural life of that community, whose "Literary and Philosophical Society" was to achieve national fame. Percival wrote extensively on medical, scientific, and philosophic subjects. In philosophy his principal interests were in ethics. His personal life was dogged by misfortunes, which, however, never daunted his spirit. A friend wrote of his *Medical Ethics:* "The admirable picture so lately drawn . . . in which he delineated the requisites and qualifications of the medical practitioner, displays the most exact portraiture of himself."[28]

The staff of the Manchester Infirmary had developed considerable internal friction, which the trustees wished to resolve. Percival, at the request of the staff, drew up a code of laws in 1792 to govern the practices at that institution. Later he enlarged the work "to frame a general system of *Medical ethics;* that the official conduct, and mutual intercourse of the faculty, might be regulated by precise and acknowledged principles of urbanity and rectitude."[29] It is important to remember, however, that this famous work was a practical guide in a concrete situation to solve specific problems. The title of the first—and original—chapter is quite unequivocal: "Of professional conduct, relative to hospitals, or other medical charities." It was designed specifically to establish greater harmony in the care of the *sick poor* and not to explore any vague ethical generalities. Yet Percival, while offering concrete rules, framed them so that they would appeal to men of good will and would harmonize with ethical sensibilities.

Percival, it would seem, felt strongly concerning the dignity and worth of the physician. The physician was indeed the great-souled man who not only must be worthy but must know his own merit. This is the basis of that *noblesse oblige* which plays a significant part in the *Medical Ethics.* In the very first paragraph Percival mentioned the condescension and authority which a physician should show the patients, the indigent sick. "Condescension"—the term suggests the aristocratic tradition so liable to corruption. "Authority"—this reflects the superior status of the physician. But authority implies also responsibility. Physicians were enjoined to consider the feelings and emotions of the patients. Percival emphasized the delicacy and tact which the attending physicians must use in various circumstances.

He also analyzed, in considerable detail, the classes of patients who should be admitted. A hospital need not accept all patients; many could come to a dispensary.

Percival was not systematic, but his discussion of responsibilities covered not only patients but professional colleagues and the institution itself. Thus, of personal doctor-doctor relationships he said, "The medical gentlemen of every *charitable institution* [italics mine] are, in some degree, responsible for, and the guardians of, the honour of each other." They must not publicly reveal anything that might injure a colleague's reputation until the complaint has been laid before the physicians of the institution who will judge its merits. The staff has a responsibility toward what we today might term "scientific progress." Percival insisted on the duty to try new remedies and new methods of treatment, but these must be "scrupulously and conscientiously governed by sound reason." Records and "hospital registers" are extremely important in advancing knowledge. He pointed out the need for committee meetings to oversee and improve various aspects of hospital work.

A most important topic concerns professional consultations, a point on which there is today much misunderstanding. Very unfortunately, Leake, introducing this edition, stressed a distinction between medical etiquette and medical ethics, between matters of "professional courtesy" and those of "ultimate consequences." Such a distinction does not accord with sound historical perspective for the eighteenth century. It is very misleading indeed to think that Percival's recommendations regarding consultations, for example, were mere trivial etiquette or vain inconsequential punctilio. Codes of medical ethics arose from practical economic or interpersonal struggles which had become so generally distressing that some concrete remedy was sought. The difficulties were *here* and *now,* and a code was in large part a practical method of eliciting order. We might point, as an analogy, to present-day automobile traffic regulations. The magistrates do not concern themselves with the philosophy underlying a "No Left Turn" sign. They do want to keep traffic moving smoothly. Those who frame regulatory codes would all admit that the Golden Rule and the Sermon on the Mount are fine ethical doctrines. A traffic code, however, is a realistic doctrine in which courtesy, consideration, and gentlemanly behavior, however desirable, do not take the place of fairly specific rulings. The same is true of codes of medical behavior, although these are more exhortatory, without the juridical force attaching to traffic rules.

In the eighteenth century professional consultation raised very trying problems, partly from economic conflict and partly from injured pride. A set of regulations governing techniques of consultation was a tremendous step forward to ease a difficult situation. Percival stressed the importance and desirability of this sharing of opinions. He also laid down certain definite rules which there is no need to repeat. Today all hospitals have their own regulations regarding consultation. They are very different from Percival's, but after all, the twentieth century is rather different from the eighteeth. It is no cause for surprise that eighteenth-century problems have today lost their urgency and today seem only quaint and archaic. In their contemporary setting they were very real.

Among miscellaneous regulations on of the most interesting concerns the care of "female patients, labouring under Siphylis." Percival recommended a "Lock Hospital . . . founded on the most benevolent principles, consonant to sound policy, and favorable to reformation and virtue." He went on, "The strictest decorum should be observed in the conduct towards the female patients; no young pupils should be admitted to the house; . . . books adapted to the moral improvement of the patients should be put into their hands . . . and, when practicable, some mode should be pointed out of obtaining a reputable livelihood."

Percival also discussed "Asylums for Insanity" and the special difficulties which they entailed. He closed the chapter with a few words on "hospitals for the smallpox, for inoculation, for cancers, etc.," which, however, required no professional duties different from those already examined.

The second chapter, entitled "Of Professional Conduct in Private or General Practice," extends to private patients the same "moral rules of conduct" laid down for hospital regulations. Much of the chapter, however, is vague and general, covering without much advance the same ground as did John Gregory: Patients should be treated with humanity. Doctors should be temperate. They should notify the family of gravely ill patients. Far more significant and interesting are those paragraphs which are concrete. He returned again to the vexing question, When and how should doctors consult? Percival devoted many pages to the various aspects of the problem. There is no need to repeat the details, but under no circumstance should these vexing problems be looked down on as "mere" etiquette.

When may a doctor interfere with a patient under another's care? This is another urgent practical problem. Percival's answer declares, "When artful ignorance grossly imposes on credulity; when neglect puts to hazard an important life; or rashness threatens it with still more imminent danger; a medical neighbor, friend, or relative, apprized of such facts will justly regard his interference as a duty."

He mentioned that apothecaries, surgeons, and physicians and their families should be attended gratuitously, as well as clergymen of straitened means. But if colleagues or clergy were affluent, "a pecuniary acknowledgement should not be declined." The concept of *noblesse oblige* emerges strongly with the idea that military or naval officers "in narrow circumstances, are also proper objects of *professional liberality*." This term recurs repeatedly. Percival maintained that everyone who "enters into a fraternity engages, by a tacit compact, not only to submit to the laws, but to promote the honour and interest of the association, so far as they are consistent with morality, and the general good of Mankind." This sense of honor and professional pride was strongly marked in Thomas Percival himself.

Since a code of ethics comes into being because of practical conflicts, usually economic, moral precepts which ignore this basis will be merely pious wishes of little force. Percival combined the ethical and the economic in a very excellent way. Speaking of professional fees, he superbly blended the ideal and the practical: "To a young physician it is of great importance to have clear and definite ideas of his profession; of the means for their attainment, and of the comparative value and dignity of each. Wealth, rank, and independence, with all the benefits resulting from them, are the primary ends which he holds in view, and they are interesting, wise and laudable. But knowledge, benevolence and active virtue, the means to be adopted in their acquisition, are of still higher estimation." This scheme of values is perhaps the most interesting generalization in the entire volume, entirely realistic yet ethically satisfying. This indeed is a concept which can lead us to thoughts of "ultimate consequence."

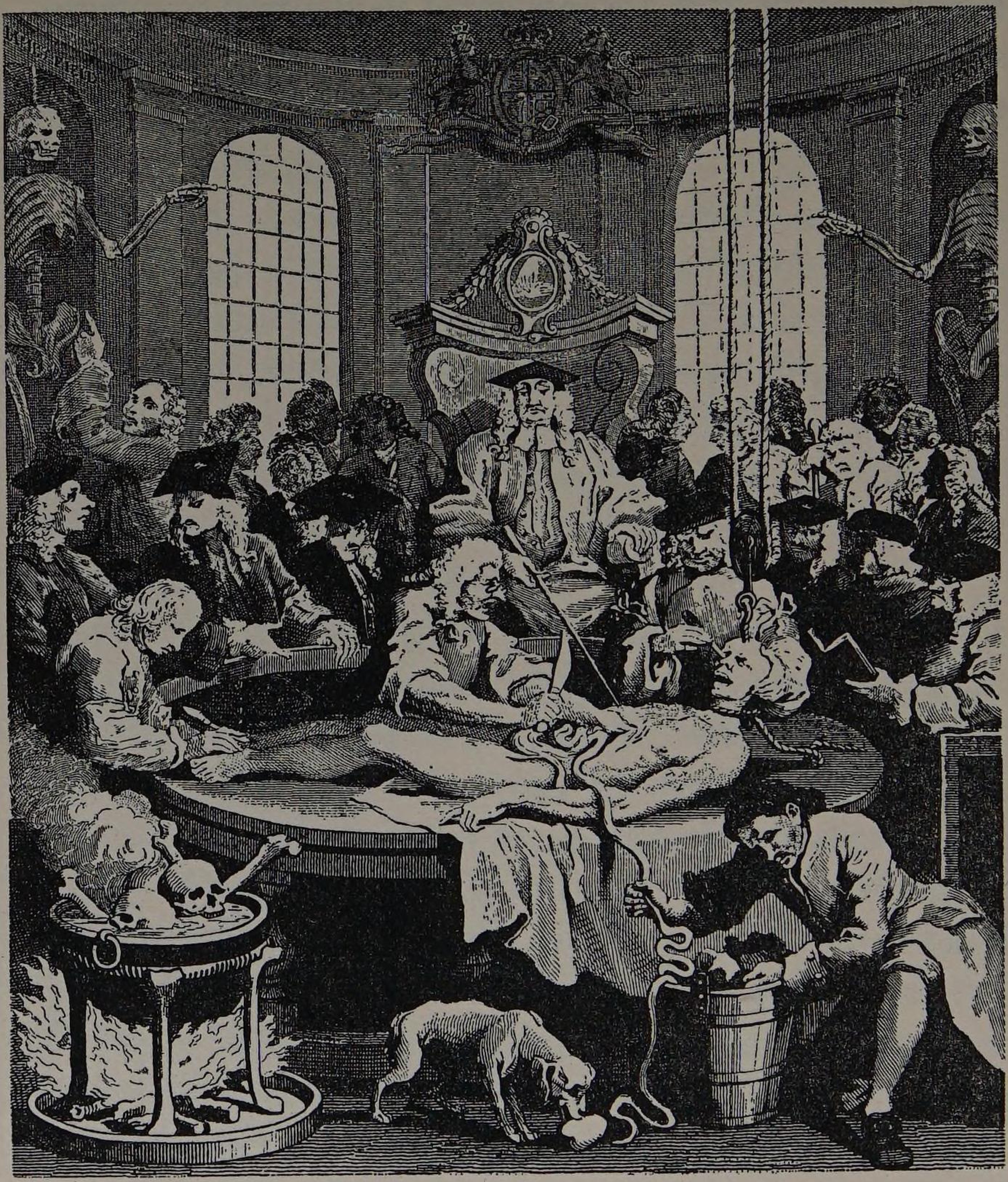
The remainder of the chapter includes directions for conduct in rather specific situations: consultations by letter, writing medical certificates of disability, the use of nostrums, the review of cases having a fatal outcome, the observance of the Sabbath, retirement of elderly physicians from active practice. Percival gave definite advice: *this* is what ought to be done. Such advice is contingent upon circumstances. Percival's code can be understood only by reference to the historical, circumstantial background.

A third chapter in the code, although brief, is very illuminating. "Of the Conduct of Physicians towards Apothecaries." The tone is very patronizing indeed. He praised the apothecary, lauded his position in society, enjoined co-operation between the two practitioners, but emphasized that the relationship was one of the superior and the inferior. Even though he have confidence in the apothecary, the physician should nevertheless inspect, from time to time, the apothecaries' drugs and should instruct him in the particular conduct of various cases. The apothecary is entitled to a fair remuneration. A physician should not visit the apothecary's patients in the latter's absence. "Physicians are the only proper substitutes for physicians; surgeons for surgeons; and apothecaries for apothecaries." Percival felt strongly that the three different branches of the healing arts should each maintain its independence. Needless to say, the physician was to be the guiding spirit and the basic authority.

The final chapter in the *Ethics* is entirely irrelevant to our purposes: "Of Professional Duties, in Certain Cases Which Require Knowledge of Law." This is an elementary text of medical jurisprudence, not at all germane to our discussion. It furnishes an interesting clue, however, to the thought processes of the era, since it formed an integral part of eighteenth-century medical ethics as Percival envisioned the subject.

Percival's code is of great historical interest from at least three different viewpoints: It helps give us a picture of the man himself, aristocratic, sympathetic, with a strong moral sense, but always keenly aware of the real problems of the day. Second, the book by its provisions indicates just what social and economic problems were important in medical practice. And finally, the little volume served as a point of departure for later compilers. It was the parent of numerous other codes appearing in the nineteenth century, especially in the United States.

The eighteenth century saw great progress, not only in medical knowledge but in medical organization and discipline. The tripartite professional division into physicians, surgeons, and apothecaries persisted throughout the entire century. There were complicated relationships of these groups to one another and to the public at large. Compelling circumstances, social, economic, and scientific, molded and changed these relationships. Such changes, definite enough in the eighteenth century, became even more drastic in the nineteenth century. What we today call "medical ethics" reflects, in small compass, all those complex and changing factors which, arising in the eighteenth century, still are influential in the mid-twentieth, still are changing, and still require rule and exhortation to promote a smooth adaptation.



IX

The Rise of Modern Pathology

The modern pathologist leads a very busy life, but only in recent years have his activities come to popular attention. Newspapers, magazines, and television have now created a glamorous version: he is the man who makes rapid frozen sections and tells the surgeon whether to amputate; or he performs various intricate tests with complicated apparatus; or, surrounded by guinea pigs, test tubes, and a microscope, he does something called "research"; or, stripped of his glamour, he performs autopsies in the morgue.

If now we ask, *Why* does the pathologist do all these things? we get two different answers. The pathologist today exerts a service function, using his special training to solve problems which other physicians, lacking technical facility, cannot solve unaided. The rapid frozen section and its interpretation, the manipulation of test tube and burette, the emergency blood-typing, all are concrete aids in particular situtions.

All this, however, is only a by-product of specialization, a late accretion to pathology and, historically considered, not very important. Originally, pathology was not distinguished by its specialized techniques. It was a *point of view,* not a distinctive activity. It marked an interest rather than a performance. Thus, one physician might rest quite content if he merely noted his patient's symptoms, made a diagnosis, and prescribed some medicines. All this would be relatively simple medical practice on a strictly empirical level. On the other hand, another physician could literally go beneath the surface. He could dissect dead bodies to accumulate new data not previously available; he could study the patient's blood, secretions, or excretions to seek more light on the disease processes. Why do all this? To gain new insight, to permit wider and more complex correlations, to engender new theories of disease (or puncture old ones). The pathologist was the man who tried to *understand* disease and who did a lot of different things to further this goal. In contrast the less sophisticated physician contented himself with empirical correlations, without seeking the how or why.

It was only fortuitous that pathologists were so closely allied to the dissecting table. During the Renaissance anatomy had made the greatest progress of all the medical sciences, and physicians relied especially on anatomical dissections to further their knowledge. The tradition so established has persisted to this day. Had chemistry made tremendous strides in the eighteenth century, sufficient to explain disease states, then the pathologist, or student of disease, would have been primarily a chemist. But as it was, the data to explain disease came principally from the autopsy dissections.

This does not mean that autopsies would always explain. They were not necesarily fruitful, nor did they always yield sound generalizations. Nevertheless, they were performed in great numbers. In the late seventeenth century Theophile Bonet (1620–89) collected all available autopsy reports recorded during the sixteenth and seventeenth centuries. His great work, the *Sepulchretum,* first published in 1679 with a second edition appearing in 1700,[1] contained almost three thousand separate cases. Bonet classified these and added considerable comment of his own. In the present chapter, however, we will restrict ourselves to the latter part of the eighteenth century, to the work of five authors who among them spanned the entire century.

One figure, virtually forgotten today, is Joseph Lieutaud (1703–80). A prominent clinician best known for his *Précis de la médecine pratique,* which was widely reprinted and translated, he also wrote a *Historia anatomico-medica.*[2] This was a massive compendium of autopsy reports analogous to the *Sepulchretum* of Bonet. Lieutaud combed the literature, particularly of the late seventeenth and early eighteenth century, and collected a total of some thirty-five hundred cases that had been published in various countries. Mostly brief, these cases each included first a very short clinical background and then the bare anatomical data.

Lieutaud had a truly modern notion of pathology. The examination of dead bodies, he declared, was a most distinguished art. Accurate post-mortem examination served a twofold aim. It disclosed the structure and relation of the parts, and the natural connections. That is, in paraphrase, it contributed to anatomy and physiology. Dissection also served to search out and expose to direct visual observation the nature of disease, which otherwise, lying hidden, could only be imagined, in futile conjecture. Pathology was indeed the helmsman of medical practice, enabling the physician to avoid the reefs and storms.

There is no doubt, Lieutaud went on to say, that accurate observation of viscera gives us knowledge of internal diseases. The real site of the disease is discovered, while causal factors are disentangled which would otherwise be hidden. Autopsies render the methods of treatment safer, prognosis more accurate. Dissection is the foundation of true medical science, while spurious doctrine founded only on "hypothesis" readily collapses. Dissection of cadavers is the key to hidden phenomena, and, he continued, only that is to be considered assured in the art of medicine which can be sensibly perceived and demonstrated in experiment.[3] In other words, the autopsy furnished solid evidence, the only firm base for reliable theory.

While the French pathologist had a sound respect for so-called facts, he could not adequately evaluate them. Lieutaud's *Historia anatomico-medica* was a tremendous but uncritical compilation, representing an aging viewpoint that was to wither before the new critical spirit pervading the late eighteenth century.

A random sampling will illustrate the general character of these case reports. For example, a man in his fifties had cardialgia and difficulty in swallowing. When the stomach was empty, a mass was palpable in the epigastrum—a tumor, hard and circumscribed, about two inches wide, seemingly continuous with the ensiform cartilage. There was difficulty in breathing and in swallowing, progressive emaciation and loss of strength, and the patient died. Autopsy showed a hard tumor larger than a hen's egg, blocking off the entrance to the stomach. There was another mass of the same type the size of a chestnut, very close to the pylorus.[4]

That is all. That is the entire report. The modern reader immediately wants to know the type of tumor, whether it arose from the mucosa or the submucosa or the muscle, or whether from the stomach at all. But these questions reflect modern discriminations not then available. Contemporary knowledge could not have yielded appropriate answers, or even suggested such analysis.

In contrast, a man of about the same age, suffering from repeated bouts of jaundice and vomiting, with pain in the region of the stomach and a tumor in the right hypogastrium, showed, on autopsy, a large liver which was almost entirely "steatomatous" within and had many nodules on the surface. The pancreas was "scirrhous," and the gall bladder was hard and "dry," with many stones occluding the cystic duct.[5] The presumptive modern diagnosis is, of course, a carcinoma of the pancreas, with metastases. The term "steatomatous" covered a wide variety of conditions. The gall-bladder disease was presumably incidental only.

We gain special insight into the eighteenth-century medical mind if we follow the total analysis of one or another organ. In this way we can understand the categories with which men worked, the divisions which they thought significant. Let us take, for example, the gall bladder.

Lieutaud, facing a mass of observations concerning the gall bladder, had to organize his data according to some guiding principle. The factor which seemed to him most important was the contents of the viscus. Variations in this factor served to establish the separate categories. In the first were cases where the organ contained thick black bile; in the second, rusty or green bile; in the third the bladder contained a "degenerated" humor, such as white bile or very black bile. Fourth was what we would call empyema of the gall bladder, where the viscus was ulcerated and full of pus. Next he distinguished bladders with unusual distention. For example, in two cases the viscus contained an alleged eight pounds of bile. The reasons for the huge distentions were not given. The sixth division included cases of gallstones (which he called "pseudo-stones"). In many cases the clinical history showed well-defined biliary colic, with or without jaundice. In other cases, however, the stones appeared to be only incidental findings without clinical significance, but all were placed in one single category. The seventh division was designated "obstructed biliary ducts." In all these cases the occluding agent was a stone. No tumors were included. The remaining categories included blockage of biliary ducts by worms, perforation of gall bladder, and "empty" gall bladder.

Today, when we claim to think in etiological terms, we establish quite different categories, such as inflammation, neoplasia, metabolic disorders. These were not clearly marked out in the eighteenth century, and the gross observation, what was *in* the gall bladder, seemed an adequate point of division. Some of Lieutaud's categories still persist in modern thinking, while others seem quite unimportant.

It is interesting to review Lieutaud's classification of heart disease and the pathology involved therein. Here is a strange mixture of categories. The multiplicity of cases required analysis, but the analytical tools were not very incisive. There was no firm understanding of basic processes. On the grounds of morbid anatomy he enumerated twenty-three types of heart disease. We must remember, of course, that this analysis depended on data recorded principally by other anatomists, observers who varied greatly in ability and insight and who presented no uniform point of view. Lieutaud, largely classifying other people's opinions, dealt principally with facts at second hand, so to speak, already seen through someone else's eyes.

These twenty-three categories of heart disease appear to be in completely random order. We obtain much better insight if we rearrange his classes into six groups. The first of these concerns the over-all *size* of the heart (his sections 1, 2, and 20). Some hearts were large and greatly dilated, others showed unusual dilatation of the auricle. Many of the large hearts were obviously rheumatic, but the descriptions are most inadequate. For example, one case, originally described by Lancisi,[6] mentions a man of twenty-five who had been ill with an "inflammatory angina," which left him with a difficulty in breathing, especially when he walked quickly. He also had deep-lying precordial distress and strong pulsations above the diaphragm. At autopsy the heart, after the blood was removed, weighed two and a half pounds and was noted to resemble a cow's heart. The only details of description, however, relate that the amount of blood in the right auricle and ventricle was small, while the left ventricle and auricle held a very large amount. There is no description whatever of the valves. Other cases, however, briefly mention some valvular changes. Still other hearts in this category probably represented hypertensive disease. In the general category of *size* we should also include his section on small hearts, observed in phthisis and other febrile diseases, which, apart from the small size, did not appear remarkable.

A second group (including his sections 4 and 5) notes the *external appearance* of the heart. In one section the hearts are described as covered with fat and in the other as having a surface covered with "crust" of obvious pericarditis.

A third major division (his sections 3 and 6) analyzes the *contents* of the heart. In several cases the cardiac cavities were empty. A large number of cases, thirty-nine in all, were distinguished by the "polypi" within the chambers. These post-mortem clots were once considered of pathologic significance and their presence carefully noted and often well described: similar to fat; like clear gelatine; a fleshy mass white on the inside, reddish outside; and so on.

A fourth major division of diseased hearts may be created from several sections in which Lieutaud, in one or another way, dealt with the *myocardium* (sections 7 to 13). These include various states which he called ulcerated, putrid, purulent, abscessed, or gangrenous. These probably represented partly post-mortem changes, partly various inflammatory changes, true myomalacia and infarction. This general subdivision may also include Lieutaud's section on tumors of the heart, describing genuine metastatic lesions.

There is a substantial group of cases, arranged in five sections (14, 15, 17, 18, 19), all dealing with *valvular disease,* which was clearly recognized as a major category of cardiac pathology. The reports described with greater or less clarity valvular thickening with or without calcification and varying degrees of fusion or destruction.

The remaining sections may be lumped together as *miscellaneous,* including wounds of the heart, "worms" in the heart, and displacement from normal position.

The whole mammoth compilation, the last of its kind, had many merits but many defects. There were thousands of case reports, by dozens of different authors, who differed in knowledge, acumen, ability, trustworthiness. All reports were equated and treated impartially, just as if the various cases were bricks stacked together to make a wall. In a finished wall one brick is very much like another in appearance and in value. But this uniformity does not apply to pathology descriptions.

Lieutaud systematically compiled material that from the historical standpoint is invaluable. The whole work, however, exemplifies the collector's instinct rather than the pathologist's insight. In the eighteenth century many learned men gathered together museums of natural history wherein all sorts of curious natural objects were collected and displayed, usually in rather indiscriminate fashion. The same collector's instinct became manifest on a much smaller scale, in the "curio cabinet" that graced many Victorian parlors. Mere collection, however, can carry us only a short distance down the path of knowledge. Lieutaud, although he gathered material, did not do very much with it. It was only raw data which eventually some more skilful and discriminating person might analyze, evaluate, and by force of genius use for a more creative contribution. Lieutaud realized the importance of pathology, but he could not bring his insight to fruition. He was a compiler, useful, but less useful than one who performs a genuine synthesis.

Giovanni-Battista Morgagni (1682–1771) is generally considered the founder of truly modern pathology. His great contribution, *De sedibus et causis morborum,*[7] published in 1761 when he was seventy-nine years old, definitely bears evidence of old age. We notice a petulance, especially in his criticisms, which are usually quite ungracious. He was highly verbose and often needlessly circumstantial. His parade of erudition exposed a not inconsiderable vanity. Nevertheless, he exhibited a critical acumen, a grasp of scientific method, and a stimulating enthusiasm which lift his book far above its contemporaries.

The pathologist, trying to understand disease, starts with clinical observations and tries to correlate these with anatomical, chemical, or other findings. If through his studies he can establish a firm correlation, so that from one group of factors he can predict some other set of factors, then he demonstrates a considerable understanding of the disease in question. If knowledge is true, it enables us to predict. Morgagni frequently illustrated this principle. There was, for example, the well-known case concerning an epidemic of lung inflammation in Padua. In a convent a nun "was seized, in the night, with a fever, with which she first shivered, and was cold . . . and after that grew hot." Then she showed pain in the side, difficulty in breathing, dry cough, a hard pulse, sense of weight in the thorax. After her death, Morgagni said, "Come, let the body be dissected; this will be certainly found to be the nature of the diease, that the lungs shall appear to have the substance of liver." At the autopsy he saw that "the lungs were cover'd with a whitish and thickish kind of membrane . . . and to the corresponding pleura . . . adher'd a reddish kind of sediment. . . . they were not only heavy, but in more than one place, hard. When they were cut into they appeared to have a dense and compact substance, like that of the liver, as I had predicted."[8] And he continued describing the pneumonia in considerable and excellent detail.

Morgagni insisted that clinical observations alone were incomplete and subject to grave error without proper pathologic background. He described, for instance, a consultation where a surgeon asked his advice concerning a patient whose tongue exhibited "many small tubercles, like warts, on the upper surface of it, very near to the basis." The surgeon queried as to the best way of extirpation. Morgagni, however, recognized that "those tubercles were nothing more than the last and largest of all the papillae," slightly affected by "phlogosis" (inflammation). He therefore bade "both the woman and the surgeon to be of good courage, and gave him no other advice . . . but that he should frequent anatomical demonstrations; by the neglect of which it happens more frequently than you imagine, that the appearances which are natural are consider'd as morbid."[9]

Although sometimes painfully verbose, Morgagni was a gifted observer who could describe accurately and vividly. Sometimes his observations were original, at other times they concerned familiar entities, described better than before. There are many brilliant word-pictures, a fine example of which concerns a "most excellent Marquis" who, suffering from aneurysm, "could neither lie down, nor go to stool, nor make water, nor even swallow nourishment, but almost immediately a paroxysm was brought on, which threaten'd instant suffocation. . . . he us'd, sometimes, to leap suddenly out of the seat . . . and run to the window with hopes of breathing better there; yet, nevertheless, he us'd to draw his breath with a stertor, to be livid in his whole face, to discharge his urine and faeces involuntarily. . . . But sometimes the stricture upon his breast was so great, that his breath being quite intercepted, he could no more even snore, but fell forwards, just like one that was dying."[10] This is a fairly characteristic example of Morgagni's vivid style.

He gave numerous anatomical descriptions of aneurysm, describing the huge tumor, the erosions of the bones of the thorax, the compression of the lung by the large sac which opened into the aorta. "This sac seem'd to be form'd of the produc'd substance of the aorta, but was more white, tenacious, and dense, and not less thick. This substance was internally invested with many curv'd laminae, folded one within another, seemingly fibrous, and really polypous, but without moisture, and these laminae contain'd such a quantity of blood within their circumference, and this almost concreted."[11] Thus did he describe the tough fibrous aneurysmal wall, continuous with the aorta, with a contents of thrombotic material and gelatinous clot.

Morgagni had his own notions of pathogenesis. In one place he declared, "I do not doubt, but that the corrosive particles that contaminate the humors of those who are infected with venereal lues . . . frequently stagnate, in the coats of the arteries . . . and weaken these coats by corroding them here and there, and in that manner, render them more liable to dilatations."[12]

It is of interest to add here his description of the valves in a case of luetic aortitis: ". . . nor was there any evident disorder in the valves, except in those which are plac'd at the mouth of the aorta, for these we saw very lank; and contracted into themselves, somewhat rigid also, and a little hard, and that particularly in the upper border of each.[13]

We could multiply examples indefinitely. But far more important than his descriptive ability was his sound critical judgment, grasp of scientific method, and ability to weigh evidence. Wrong he frequently was, but he constantly gave the impression that he had duly considered the available evidence and had not jumped rashly to a conclusion.

Pericardial effusion is a case in point. Dropsy of the thorax—that is, hydrothorax—was a familiar enough condition, moderately well understood, but dropsy of the pericardium was rather baffling. Reported cases were few; his own personal experience was small. He examined the data, trying to correlate proven fluid in the pericardium with some particular clinical manifestations, so that either might be predicted from the other. He sought a "sign so far proper to the dropsy of the pericardium, that this dropsy could not exist without the sign, nor the sign without the dropsy." But no such pathognomonic sign could he find. The diagnosis was at best contingent. But he did believe that there were a number of findings sufficiently suggestive that, if "all these, or many of them at least, shall be found together; I indeed [think] . . . although I will not for certain pronounce, that the disorder is a dropsy of the pericardium, yet shall say that it is extremely probable."[14] This cautious, reserved, critical attitude contrasts sharply with the more customary dogmatism that flourished then, and still does. He realized that not one single sign, but rather many different factors combined together, must point the diagnosis, which was to be considered only probable. His teachings of scientific humility have not yet been fully learned.

The chapter on "palpitation of the heart" is quite illuminating. Morgagni wished to find the cause or causes of that particular state. He collected all the available material wherein palpitations were mentioned and noted any accompanying pathology which was described. Many different abnormalities had been recorded in association with the palpitations, such as hydrothorax, bony or calcified heart valves, dropsy of the pericardium, worms in the pericardium, air, or flatus, within the pericardium, adhesions between heart and pericardium, "tumors" of the pericardium (chiefly cysts or possibly abscess). Various hypothetical changes in the nervous system had also been implicated. Each of these had been assumed by one author or another to cause palpitations.

Morgagni realized that a pathologic state cannot be accepted as causal unless there are adequate controls. He observed that alleged associations between pathologic findings and the palpitations were not constant. He particularly attended to such factors as valvular disorders and pericardial adhesions. We form some idea of the difficulties in evaluation when we realize that, in the matter of pericardial adhesions, Valsalva had described only three cases and Morgagni himself—already an old man—had seen but seven. Of these seven, in four it was impossible to learn whether palpitations had been present during life, and of the other three he felt assured that palpitations had been present in only one. Hence Morgagni criticized those authors who thought that palpitations ought "naturally" to be the consequence of such adhesions.[15] When there was little evidence available, it was very easy to jump to conclusions, and a judicious reserve was all the more praiseworthy.

He treated carefully even such presumed factors as worms in the pericardium, a condition whose existence he doubted. Although he showed the evidence to be unsound, he maintained a judicious attitude. His rejection was not "by way of obstinate contention that it is impossible for a worm even to exist within the membrane of the pericardium . . . but I fain would read more certain observations before I allow it."[16]

Such caution is praiseworthy. Consonant with the ideas of the great French clinician Senac, he pointed out, ". . . when the signs of . . . disease are ambiguous or obscure, then physicians must take great pains . . . in order that they may be . . . cleared up . . . and that though they [the physicians] should not be able to attain to them, yet they might, in the meanwhile, be reaping this advantage . . . that they would naturally become more cautious, in pronouncing upon diseases, and in determining the method of their cure; and by this means greatly improve the physical science."[17] Despite the obscurity of the pronouns, the exhortation is admirable.

We have mentioned that pathology, at its origin, could be considered an attitude of mind, a search for inner connections within the realm of disease. Special techniques and specialized information represent only a later addition.

Morgagni represents pathology at its early, untrammeled beginning.

It is well to point out that *autopsy,* the term so inseparably associated with pathology and commonly taken to mean the post-mortem dissections, originally had a quite different significance. The term derives from *autos* (self) and *optos* (seen), that is, what is seen for one's self. The autopsy represents what has been personally examined. Seeing for one's self is the first stage in wisdom. Accumulated knowledge is obviously very important, but no amount of erudition or knowledge of the past can replace personal observation, well disciplined and critically undertaken. The observations of others must be integrated with one's own. The good pathologist, seeking the hidden causes of disease, must have this critical attitude. He must be able to select, and select wisely; he must judge, and judge fairly; he must evaluate, and evaluate logically. All these processes we can sum up as constituting *judgment.* Morgagni rose head and shoulders above his contemporaries in this matter of judgment.

Morgagni especially opposed the dogmatism of contemporary medicine. Perhaps the best summary of his way of life we find in a passage where, after mentioning that dogmatism is easy for the ignorant, he pointed out that those "who have dissected or inspected many bodies have at least learn'd to doubt, when the others, who are ignorant of anatomy, and do not take the trouble to attend to it, are in no doubt at all."[18]

Morgagni, as a pioneer, made numerous false turnings and often lost his way. This is the lot of the trail-blazer. Those who come after have an easier path to follow. A conspicuous follower in the path Morgagni blazed was Matthew Baillie (1761–1823), who wrote the first English text on pathology. He had unusual opportunities to study the subject. The nephew of William and John Hunter, young Baillie became a physician almost as a matter of course. At the age of eighteen he went to London to live with his uncle William. This close contact with the Hunters, together with his more formal training, combined to give him probably the finest medical education his world could offer. In 1787 he received the degree of M.B., in 1789 the M.D., and he very soon became a fellow of the College of Physicians and of the Royal Society. His book, *Morbid Anatomy,* which he published in 1793, achieved a great and well-deserved popularity, with several editions at home and abroad.[19] Later he issued a series of engravings to illustrate the text.

Earlier works dealing with pathology, said Baillie, had certain defects. Even Morgagni, by far the greatest of his predecessors, exhibited certain faults. The descriptions, said Baillie in typical understatement, were often too general and their very bulk impaired their usefulness, while they "have been sometimes obscured, by taking notice of smaller collateral circumstances" which had very little relevance.[20] Baillie changed the traditions. He gave virtually no case reports and omitted most clinical details. He did not go, pedestrian-fashion, from one case to another but abstracted from many observations a precise description of morbid entities, to define a number of distinct morbid states.

In his preface Baillie struck a very dynamic note. He claimed that he was trying to explain "the changes of structure arising from morbid actions in some of the most important parts of the human body." There was a wide gap between "action" and "structure," between function and morphologic appearance, between diseased function and morbid anatomy. Baillie felt that, by studying altered structure, "we shall be more likely to make some progress towards a knowledge of the actions themselves, although it must be very slowly."[21] He implied, that is, a certain primacy to the functional aspect, implied that morbid anatomy was valuable chiefly as it contributed to the understanding of morbid action.

Despite these fine words, he actually made very little effort to explain anything at all. He was content to describe, to remain close to the immediate facts, not going beyond his data, not caring to analyze, correlate, theorize. Speculation, of course, had been the curse of earlier medicine. Physicians leaped so far beyond their data that they fell into the quicksands of "hypothesis," which almost all previous authors inveighed against, only to fall into the same morass themselves. This Baillie intended to avoid. He felt that "attentive examination" of morbid structure would lead to sharper discrimination between disease entities, allowing us to distinguish diseases with greater accuracy, to extend our knowledge to "parts which are but little, or not at all known," and perhaps aid our knowledge of remedies. His goals were thus relatively modest.

In the field of descriptive morbid anatomy, Baillie's delineations merit the highest praise. To choose a few fine examples:

> Opportunities occasionally offer themselves of observing ulcers of the stomach. These sometimes resemble common ulcers in any other part of the body, but frequently they have a peculiar appearance. Many of them are scarcely surrounded with any inflammation, have not irregular eroded edges as ulcers have generally, and are not attended with any particular diseased alteration in the structure of the stomach in the neighborhood. They appear very much as if, some little time before, a part had been cut out from the stomach with a knife, and the edges had healed, so as to present a uniform smooth boundary round the excavation which had been made. These

> ulcers sometimes destroy only a portion of the inner coat of the stomach at some one part, but occasionally they destroy a portion of all the coats, forming a hole in the stomach. . . .[22]

Simple, objective, accurate, with no theoretical overtones, but nevertheless sterile until someone can connect these observations with "morbid action."

His description of cirrhosis was superbly simple. He described it as the "common tubercle" of the liver. It was frequent in those of middle or advanced age, more common in men, and found most often in "hard drinkers." These tubercles

> occupy generally the whole mass of the liver, are placed very near each other, and are of a rounded shape. They give an appearance everywhere of irregularity to its surface. When cut into, they are found to consist of a brownish or yellowish-white solid matter. They are sometimes of a very small size, so as not to be larger than the heads of pins, but most frequently they are as large as small hazel nuts. . . . When the liver is thus tuberculated, it feels much harder to the touch than natural, and not uncommonly its lower edge is bent a little forward. Its size, however, is generally not larger than in a healthy state, and I think it is often smaller.[23]

He went on to mention the bile discoloration, the fluid in the abdomen, and the jaundice. This whole condition he called a "scirrhous liver," but since it bore only a remote resemblance to scirrhus seen elsewhere, he considered it a peculiar disease affecting the liver.

He gave an equally good description of metastatic carcinoma in the liver which, however, he called "large white tubercle of the liver." He mentioned the variable size and disposition within the liver; the "firm, opaque, white substance . . . generally somewhat depressed, or hollow"[24] on the superficial surface. He noted that this was much more rare than the preceding and that it resembled scirrhus in other parts of the body.

In general Baillie was quite logical when analyzing the pathologic states of different organs. He distinguished inflammatory states, thickenings, hardenings, softenings, ulcers, tubercles, scirrhus formations, cysts or hydatids, calculi, hernias, aneurysms, malformations, and the like, the list obviously varying from one organ to another. The subdivisions, all based on gross appearance, were quite sharp and usually mutually exclusive. But the analysis in general was remarkably static. Only rarely was there a dynamic approach, a feeling of process or becoming, of organic unity.

The second and subsequent editions did present, by way of improvement, sections on "Symptoms" for each chapter. He tried to give, briefly, the principal symptoms which during life might identify the particular morbid change he was describing. This helped a little, but the book remained dry and pedantic, its dogmatism due in part to the absence of concrete material. Compared to Morgagni with his wordiness, Baillie was very concise. Yet he was without that certain vividness which helps us to relive the difficulties and uncertainties, errors and pitfalls, which attend the living patient and which form the very essence of clinical work.

Baillie's text had admirable didactic qualities. Beautifully clear and precise, short, accurate, logical, it presented a large amount of abstract data. He was a sharp observer and an excellent writer, but he lacked the vital stimulating spark of the investigator, of the synthesizer who offers a genuine insight to his reader. Baillie himself was primarily a clinician and a very excellent one. His book, written relatively early in his career, was, so to speak, a précis of pathology, an outline very helpful to the practical clinician. It was a splendid advance, from the practical standpoint, and made sound pathologic descriptions widely available in the English language. But it did not significantly widen the horizons of pathology. It presented data but did not utilize the data in forward fashion. Its accuracy was high, but it missed inspiration. A broader, more imaginative approach may commit many blunders yet be truly seminal. Baillie was an excellent physician, and his book set the pattern for modern textbooks of pathology, but he was not in the first rank of scientists. To be a great scientist it is not enough to be merely an accurate observer.

John Hunter (1728–93) was undoubtedly one of the greatest medical scientists of all times, but as a youth he showed little promise. Talent for academic studies he had none. Instead, as he himself said, "I wanted to know all about the clouds and the grasses, and why the leaves changed colour in the autumn; I watched the ants, bees, birds, tadpoles, and caddis-worm; I pestered people with questions about what nobody knew or cared anything about."[25] This was indeed the key to his life. His curiosity was insatiable. No one knew the answers to his questions; so he devoted his life to finding out the answers himself.

His formal schooling was slight. At the age of twenty he went to London, where he assisted his brother William, showed great talent for anatomy and surgery, and demonstrated himself to be a fine original investigator. After some twelve years in London, he went on the Belle Isle expedition in 1761 as staff surgeon with the royal fleet, returning to London and the practice of surgery in 1763. His great interests, however, were in the biological sciences —human and comparative anatomy, physiology, pathology, and physiologic pathology. His great museum and natural history collections, his menagerie, his experiments, were his principal concern, and not "that damned guinea" which he had to earn as a surgeon to support these other activities.

John Hunter was, by the standards of his time, a relatively uneducated man. Of classical learning he had virtually none; of conventional erudition or thorough knowledge of the great medical writers, even of the recent past, he gave no evidence. In his writings the footnotes do not bristle with learned references. Instead, there is constant reference to evidence, experiment, data, usually his own, occasionally those of others. He exemplifies the view that progress lies in looking to the present and the future, not to the past. It is as if he thought, "Most of what has been written in the past is not worth knowing, and most of what is worth knowing is not yet written. Let us try to find out." His famous letter to Jenner is characteristic: "I thank you for your experiment on the hedge-hog; but why do you ask me a question by way of solving it? I think your solution is just, but why think,—why not try the experiment? Repeat all the experiments upon a hedgehog as soon as you receive this, and they will give you the solution."[26] Neither speculative analysis nor reference to authority could give the answers Hunter sought. But only experiment. Try it and see. Why stop short with opinion when experiment can give you a far greater certainty?

This attitude can, indeed, go to extremes. The mind that is too restless dissipates itself too widely. Experiments are fine, but sometimes a little more preliminary thought, a little deeper theorizing, will improve the experiment and allow more rigorous conclusions. Nevertheless, Hunter, without any self-consciousness, performed a great service. He helped to usher in that great period in medicine which quite overturned the old concepts and theories and introduced new ideas and new methods. The first part of the nineteenth century did indeed mark a great medical renaissance. It is as if Hunter sensed what the future was going to bring, as if he tried to rush forward to meet it, as if he realized that, in his forward struggle, the dead hand of the past was mostly a hindrance. Modern criticism can better evaluate such an attitude, can discern the good and the bad much more clearly. With all his faults Hunter was a pioneer in the new movement which was to reach fruition a generation or so after his death.

Primarily Hunter was not so much a medical scientist as he was a "naturalist." This word has now become old-fashioned, for today knowledge has so far advanced that any serious investigator must specialize, and anyone who ranges over a very wide field is necessarily superficial and dilettantish. Not so two hundred years ago. Then, indeed, the gifted could take as their province perhaps not "all knowledge," but certainly all nature, including both the plant and the animal kingdoms. Hunter was deeply interested in general biology, and in those days biology meant principally anatomy, taxonomy, and general physiology. Since he was also medically trained, it was only fitting that disease should comprise a large share in that "nature" which he was investigating; yet it was only a share, not the whole, of his interests.

Hunter concerned himself not so much with disease entities, or syndromes, as he did with general reactive processes. The pathologists of the day dealt principally with whole organs and the correlation of particular diseases and particular organ changes. Hunter made great contributions in the field we now call "general pathology," that is, those basic changes with which the body responds to injury or disease of whatever sort. Inflammation and repair, degeneration and necrosis, are fundamental general reactions then just beginning to command appreciation.

In this field of general pathology, his *Treatise on the Blood, Inflammation, and Gunshot Wounds* is truly a classic.[27] When we study his actual writings, we indeed wish that his education had been better. As one editor declared, "It is to be regretted, that the style of Mr. Hunter is inelegant, and frequently obscure."[28] He could write clearly enough up to a point, but from time to time he would envelop his thoughts in impenetrable clouds. Nevertheless, his *Treatise* embodied a great deal of contemporary doctrine as filtered through his own mind and enriched by his own experiments and interpretations. Let us consider parts of his doctrine on inflammation.

In wounds, he declared, the simplest and most favorable union is by the first intention, and the extravasation of blood is a direct help to this process. Restoration "consists first in the coagulation of the extravasated blood between the ruptured parts, laying, as it were, the foundations of union";[29] then the ruptured vessels are closed off. There is an "inosculation" (restoration of circulation) although Hunter did not know how this came about. And then superfluous blood is absorbed. The blood, he taught, is "alive." After extravasation it can become "a part of ourselves. . . . The red particles are absorbed, and nothing but the coagulating lymph is retained, which being the true living bond of union, afterwards becomes vascular, nervous, etc."[30]

As a corollary, he condemned contemporary practices of cleaning out all the blood clot from a wound by way of treatment. Such interference promotes inflammation. Leaving the clot alone and allowing spontaneous resorption promotes healing by the first intention. If, however, for any reason the process cannot proceed directly to union, a new and secondary operation of nature, inflammation, will supervene. Healing then occurs not by the first intention but by the second intention. Inflammation is nature's general restorative process, a method of restoring injured parts to a "natural mode of action." It "is not to be considered as a disease, but as a salutary operation, consequent either to some violence or some disease."[31]

There was great confusion on this point, because Hunter could not distinguish clearly between septic and aseptic inflammation. In various parts of the text he regarded inflammation as a cause of disease; as a mode of cure, producing a resolution; and as a disease itself, when it took place without any visible cause. From different standpoints he thought that inflammation might arise from disease or cause a disease or be itself a disease or cure disease.

The confusion developed in part from the intrinsic difficulty of the subject, in part from Hunter's muddled style and sheer inability to deal with language. He might present lucid exposition for a short while and then embroil himself in contradictions and obfuscations. But certain concepts stand out. There is a relatively simple form of inflammation, characterized partly by its local effects of pain, swelling, and redness, partly by the conditions under which it arises. It is found in wounds and also in irritations which do not actually destroy the texture. It is this type of inflammation which is healing and reparative. Inflammation of this sort, directly curative, he considered to be "simple" and not truly a disease.

On the other hand, inflammation may arise without external violence "from a particular disposition in parts themselves, as boils arise spontaneously."[32] Quite similar are more systemic affections, such as exanthemata and internal inflammations. He was struggling to bring order into certain striking facts: (1) Inflammatory changes, which he clearly recognized, sometimes arise as secondary to a clear-cut injury or irritation. These are local phenomena, and the "cause" is obvious. (2) At other times, there are local inflammations, such as boils, with no obvious "cause." (3) At still other times there are severe generalized and systemic disorders, in the course of which there are various local inflammatory changes.

Today we are able to account for these data fairly satisfactorily through the concepts of specific infections.

But Hunter knew nothing about specific infectious agents. He was not able to achieve a satisfactory analysis. He distinguished the "healthy" principle of inflammation, as a restorative action, and the "unhealthy" principle, which showed great variety corresponding to the numberless variety of diseases. As explanatory principles he fell back on a great variety of terms: disturbed actions, poisonous qualities, specific fever, irritating causes, specific dispositions, specific diseases, unhealthy constitutions, nature of the parts, peculiarities of the constitution and a variety of "actions" and "powers." These all referred partly to infectious agents, partly to modes of response.

It is no reflection on Hunter that he could not anticipate the whole science of microbiology. However, he definitely was at his worst whenever he tried to make large generalizations or over-all explanations. Nor did he deal successfully with abstract properties and subtle concepts. He was at his best when he described concrete events or when he made limited and *ad hoc* experiments. He described three types of inflammation, the adhesive, the suppurative, and the ulcerative. Especially good is the long chapter on adhesive inflammation. He described clearly and well the reactions of the vessels, the redness, the swelling, the pain, the heat, the role of the "coagulating lymph," the edema, the thrombosis in vessels, the state of the blood, the increased sedimentation rates, the "sympathy" by which reactions might take place at a distance from the local changes, the constitutional effects. This "adhesive" inflammation included not only the simple responses of wound healing but also various infections of "erysipelatous" form and the varying types of peritoneal and pleural infections.

The suppurative inflammation he thought to be a rather different process, something above and beyond simple inflammation. It is a mode of resolution, he declared, but undesirable, representing a severe change, qualitatively different from the simple or adhesive inflammation. If the simple inflammation cannot bring about healing (by the second intention), then suppuration will supervene. Teleologically, adhesive inflammation is intended to prevent the suppurative, and the latter is always preceded by the former. The suppurative process within the cellular membrane takes place gradually. The vessels, said Hunter, "begin to alter their disposition and mode of actions, and continue changing till they gradually form themselves to that state which fits them to form pus; so that the effect or discharge is gradually changing from coagulating lymph to pus."[33] An abscess is "ripe" only when most of the coagulating lymph has been changed to pus. Suppurative inflammation elsewhere generally follows the same pattern. Hunter discussed the inflammations of mucous membranes, such as the gonorrhea, which he regarded as suppurative although clearly different from an abscess.

We should point out parenthetically that Hunter anticipated Bichat in distinguishing certain general tissue types and their different reactions. The "cellular membrane in general, together with the whole circumscribed cavities," that is, the loose connective tissues of the body and the serosal surfaces, he placed in one category, in which characteristically occurred the adhesive type of inflammation. But "all the outlets in the body, commonly called mucous membranes," including the alimentary canal, he placed in a different category, in which was primarily a suppurative rather than an adhesive reaction.[34]

In contrast to true suppuration Hunter indicated that there were collections of "matter" not associated with common inflammation. This was more of a "scrofulous" disorder, where the matter was formed independent of true inflammation.

Hunter devoted considerable attention to the nature of pus. It is not found in the blood, "but is formed from some change, decomposition, or separation of the blood, which it undergoes in its passage out of the vessels. . . . The formation of pus consists of something more than a straining of juices from the blood. . . . For producing this effect, either a new or peculiar structure of vessels must be formed, or a new disposition, and of course a new mode of action, of the old must take place. This new . . . disposition . . . I shall call glandular, and the effect, or pus, a secretion."[35]

The whole field of suppuration gave admirable scope for Hunter's experimental talent. There were many experiments reported, dealing with the appearance, properties, and formation of pus under various circumstances and time sequences. Pus was known to be composed of "globules," visible under the microscope and swimming in a fluid. But how these differed from other globules, what effect could be produced by different agents, what effects pus could exert, what physical properties it had, what chemical properties, what reactions it could enter into, these and many other questions gave opportunity for experimental study.

Hunter was not very successful in his experiments. He did not achieve any true insight into the nature of the function of pus. Indeed, he declared lamely, "I am apt to believe that we are not yet well, or perhaps at all acquainted with its use."[36] But he tried. He experimented. He reasoned carefully on the facts at his disposal. He made considerable progress. We must not blame him for not making more progress.

The *Treatise* covered tremendous ground, which it is scarcely necessary to review completely. It embodied clinical, physiologic, and pathologic studies which, in one or another form, had engaged Hunter for over thirty years. There is not very much cohesion among the different sections. The discussions of blood and of gunshot wounds might better have been separate treatises, but taken as a whole the work presents a magnificent picture of a great man. The fragmentary analysis given above presents only a distorted picture. The point of view, the fresh approach, are very stimulating. The constant recourse to experiment, of which only a very imperfect suggestion is given here, marked a new high point in medical science. The grasp of general pathologic principles, combining normal and morbid physiology with anatomy, eventually changed the entire course of pathology. Hunter might have been surprised to find himself described as a pathologist. But he does conform admirably with the concept of pathologist given earlier—one who tries to understand disease. Hunter's contributions concerned general disease processes rather than specific maladies. We therefore honor him as a pioneer in general and experimental pathology.

One of the great geniuses of medicine, Xavier Bichat (1771–1802), produced a lifetime of work within a short span of years. His entire medical career—student, surgeon, investigator—covered only eleven years, during which he worked and wrote prodigiously. He was part of that stupendous ferment which gripped all France at the end of the eighteenth century and which revolutionized not only the whole political, social, and economic sphere but the medical sciences as well. Although ordinarily Bichat is grouped with the brilliant French physicians of the early nineteenth century, he lived almost entirely within the eighteenth century and may properly be considered part of that period. Most of his ideas developed within the temporal confines of the earlier century and grew directly from the ideas of that era.

In pathology, attention had generally been directed to organs as the seats of disease, but the traditional past was no longer satisfactory. Hunter, we have seen, had studied general pathologic reactions rather than specific organ changes. On the Continent, Pinel also had stressed, in such states as inflammation, the tissues affected rather than the entire organs. It was Bichat, however, who made the greatest strides, turning attention away from organs as a whole and toward their constituent parts. He really founded the science of histology, that is, the science of tissues. Today we associate this subject with microscopic studies, but Bichat did not utilize microscopic analysis. He used the scalpel and his keen powers of observation; he performed numerous experiments, subjecting the tissues, as he said, to desiccation, putrefaction, maceration, boiling, and the actions of acids and alkalis.[37] These helped define the distinct properties and the peculiar organization of each tissue. Well-chosen animal experiments and clinical observations helped him characterize the different tissues functionally as well as structurally.

There are many different tissues, each differing from the others in its properties. In any organ, he thought, one tissue may be diseased while another remains sound. Thus, the heart may be intact while the pericardium is inflamed; the bronchial mucosa may show catarrh while the pleura is unaffected; in the stomach the mucosa may be inflamed, the serosa not. Organs are not simple entities. Their component tissues fall into certain categories, such as the "cellular" (loose connective tissues), the arterial, venous, muscular, mucous, serous, synovial, and glandular. Bichat wished to study the changes peculiar to each of these categories or systems, in whatever organ they might occur.[38]

Bichat's first book, the *Traité des membranes,*[39] published in 1799–1800, distinguished certain simple membranous tissues as well as certain pathologic membranes. Discussing mucous membranes, for example, he distinguished those of the gastro-intestinal tract from those of the uro-genital system. He considered mucous membrane a protective barrier against foreign bodies, comparable to the skin. A mucous membrane exposed to air, he noted, will have a protective power against inflammation, although a serous membrane, similarly exposed, will not. The mucous membranes produce a secretion and when irritated, produce very abundant mucus. These membranes, he thought, are excretory organs through which pass certain residual decomposition products carried there by the blood.[40] The diseases of mucous membranes he treated rather cursorily, raising far more questions than he answered. This in itself is significant. He had embarked in a new direction, and in so doing he encountered problems that had not bothered his predecessors. It is far more valuable to raise new issues and leave them unanswered, waiting for further evidence, than it is to offer premature speculations or dogmatic, unsupported solutions.

The serous membranes, thought Bichat, include not only those of the great body cavities but also those of the synovial surfaces. These all differ from the mucous membranes in structure, function, and reactions and occur in places exposed to continual rubbing and contact. They are constantly moist, since their "exhalant" and "absorbent" vessels are constantly acting. He believed that these fluids, similar to lymph, serve to prevent adhesions.

Bichat gave an illuminating analysis of organ structure. He believed that all organs possess a fibrous stroma or framework of "cellular" tissue. Within this frame in each separate organ a particular material is deposited which conveys specificity: for example, in cartilage it is "gelatine"; in bone, "gelatine" and calcium phosphate; in muscle, "fibrine." A further component of all organs is the vascular system, which brings the particular nutritive material; and as a fourth component there are the nerves. Organs are distinguished one from the other by their "nutritive material." Thus, he declared, "A bone would become muscle, if, without changing its texture in any way, nature had granted it the power of secreting and investing itself with fibrine, rather than that of separating out and incrusting itself with calcium phosphate."[41]

The serous membranes Bichat recognized as part of the framework, without any distinct "nutritive material" of their own. Not parenchymatous in nature, they represent a condensed form of ordinary "cellular" tissue. The serous membranes circumscribe and isolate the different organs and seem to facilitate movement. They possess exhalant and absorbent pores but do not really possess a blood supply. These membranes are very susceptible to irritations and "sympathy," which means the power of provoking some type of distant reaction without direct connection. Irritation or other stimulus in one part can induce extensive reaction elsewhere, whether contiguous or remote. Bichat discussed many different forms of this sympathy, needless to summarize here.

He pointed out the parallelism between adhesions of the serous membranes and the healing of wounds by first intention, both the result of inflammation. He noted similar parallelism between suppuration in the serous surfaces and suppuration in ordinary non-united wounds. Because of these similarities in behavior he argued a similarity in structure between the serous membranes and the "cellular" tissues. The latter he recognized as the essential agent in the healing of wounds. Thus, while Bichat's observations were much like those already made by Hunter, Bichat proved able to relate them in more orderly and fruitful fashion.

The third of the "simple membranes" was the fibrous type, which we now call "dense collagenous tissue." Bichat identified this material as forming the periosteum, the sclera of the eye, the tunica albuginea testis, the various fibrous capsules and aponeuroses and the like. These all have a base of tough, resistant, non-contractile fibers, hard, elastic, insensitive, but readily distensible if the distention occurs slowly.

Bichat discussed the healing of wounds and the formation of scars where membranes seemed to be involved. He restricted himself, in this work, to the healing of surface wounds involving membrane formation. Making experimental wounds, he was concerned with that "delicate film" which replaced the original skin that had been removed. He regarded the epidermization consequent on surface wounds as a membrane formation and distinguished five stages: there was the initial inflammation; the stage of granulation which developed on the raw surface; to this succeeded suppuration; the wound became smooth; and, finally, became covered by a delicate film, first red, becoming whitish. His discussion of this process is quite instructive.

Inflammation, which begins as soon as the wound is made, disposes the region to the development of granulation. Each part of the divided tissue takes on new vital activity and becomes the center "of a small circulatory system, independent of that of the heart."[42] Then small red elevations appear which tend to grow and unite and soon form a membrane exactly equal to the wound area, forming as it were, an envelope. This Bichat regarded as a "provisional epidermis," a membrane which differs from "ordinary membranes" only in having a rough, unequal surface. These granulations come from the "cellular" tissue, a conclusion he derived from the following reasoning: The granulations have the same nature in whatever organ the wound might be, whether muscle, cartilage, or skin, and therefore must be produced by that tissue which is common to all of them, the basis of all the organs. This is precisely the "cellular" tissue, which he thought to be composed of interlacing exhalants and absorbents. He believed that there was a strict analogy between inflamed serous membranes and the red film of granulations in wound healing.

This rough red film becomes transformed into a scar through the stages of suppuration and smoothing-out. The granulations he regarded as a form of secretory organ which separates out from the blood a whitish fluid called "pus," but instead of remaining in the tissues and impregnating or incrusting them, as does, for example, calcium phosphate in simple fracture repair, the pus is excreted—rejected, so to speak. The delicate layer which covers the surface granulations he compared to the membrane which covers an inflamed pleura or peritoneum—our modern fibro-purulent exudate. This whitish pus fills up the interstices, or "cells" of the granulations, which gradually smooth out. The pus becomes less and less abundant, more "laudable," and soon ceases to flow. Bandaging, he went on to say, may be more harmful than useful, and wounds exposed to air seem to heal better than those which are covered. In the last stage of healing the small cell-like spaces between the granulations collapse, the small fleshy lumps disappear and are replaced by a smooth surface, an extremely delicate membrane, much smaller than the original size of the wound.

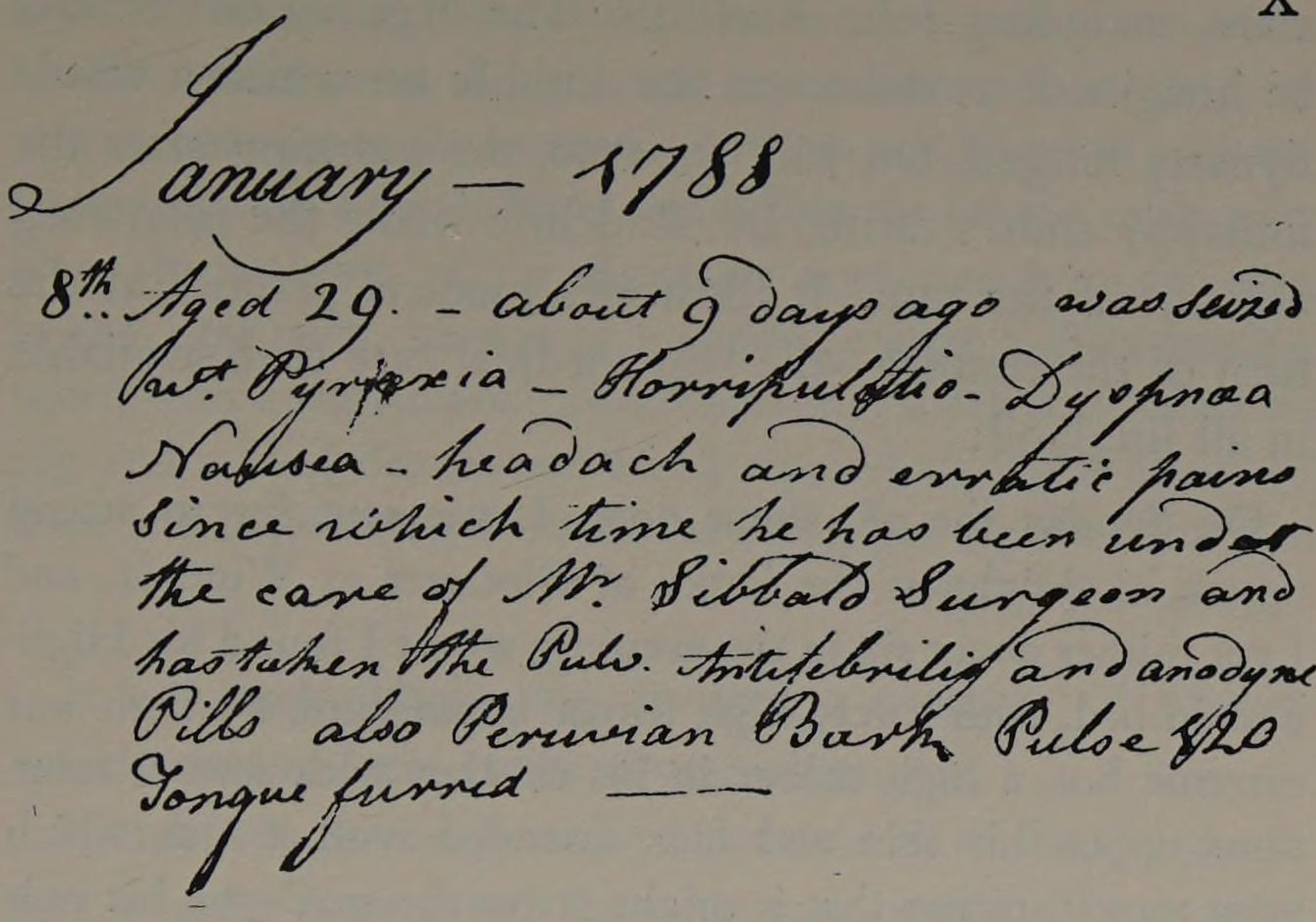
Bichat made no attempt to compare his own views with those of other writers such as Hunter or Louis; he left any such comparison to the reader to make for himself. We cannot follow in detail Bichat's various specific teachings in the whole field of pathology. The partial analysis given above illustrates his modes of thought.

In this early book Bichat systematized the study of general pathology and placed it on a modern footing. We need only compare his analysis of inflammation with the teachings of Boerhaave, described earlier. The great Dutch physician possessed well-defined ideas on the subject, but these ideas were derived deductively from the general principles of his system. They were highly imaginative and speculative, elaborated at great length from a few observations. Bichat, on the other hand, observed carefully, experimented, compared, analyzed, experimented some more, and tried to keep his inferences close to the observed facts. Theorize he did, and amply, but he did not elaborate any all-embracing hypotheses that aimed at systematic unity as the prime desideratum.

His great contribution was that medical phenomena could best be explained if we attend to the component parts of organs. In so doing, we study the reactions of tissues which are widely distributed, reactions which are common to many organs and to many diseases and which exhibit a primacy for the understanding of disease. This is *general pathology,* still the foundation of present-day medical teaching.

Bichat and Hunter belong in the same category. Both were perceptive and imaginative, top-notch scientists, masters of scientific method. That they were very often wrong is of no great importance for our analysis. They complement the genius of Morgagni. The latter, essentially a morbid anatomist who still held to organ pathology, contributed superb clarity, precise attention to detail and close clinical correlation within his field. Bichat and Hunter should be classified as experimental pathologists who attended especially to the problems of general pathology. This discipline merges with descriptive morbid anatomy and dynamic functional pathology to constitute modern pathologic science. And for this blend Morgagni, Hunter, and Bichat laid the solid foundations.

X



January – 1788

8th. Aged 29. – about 9 days ago was seized wt. Pyrexia – Horripulatio – Dyspnœa Nausea – headach and erratic pains since which time he has been under the care of Mr. Sibbald Surgeon and has taken the Pulv. Antifebrilis and anodyne Pills also Peruvian Bark. Pulse 120 Tongue furred ——

The Practice of Medicine

King James II of England embraced the Catholic faith; his daughters, Mary and Anne, remained Protestant. The Revolution of 1688 insured the Protestant rule under Mary and her husband William, but since these monarchs did not have any living children the succession seemed destined to pass to Anne. As the seventeenth century ended, Mary had died, William was ill, and Anne was soon to become queen. She was indeed a very fertile woman, bearing thirteen children to her husband, George of Denmark, but only one child survived, the Duke of Gloucester, who was the heir apparent and the last Protestant in the direct line of Stuart succession.

In 1700 the child was eleven years old. During his birthday party he became sick and grew rapidly worse. The case was desperate. The Court summoned leading physicians, including John Radcliffe. The urgency can readily be imagined: at stake was the one life on which a whole dynasty hinged. On July 30, 1700, the morning after the unhappy child's death, Dr. Radcliffe wrote the following account of the case.[1] It illustrates medical practice at the turn of the century—the best medical care then available in all England:

> On Sonday the 28. about noon I was sent for to attend uppon his Highness the Duke of Glocester at Windsor, and I got thither about six in the evening, where I found his Highness in bed, with a very high feavor uppon him, his flesh was extreme hot, a high colour in his cheekes with several eruptions uppon his skin and face, attended with a rash which gave som suspicion that it might prove the smal pox his puls was very quick and feavourish his tongue white, and his swallowing without paine or difficulty his breathing by fitts short and attended with very great and frequent sighing. . . . And he himself very restless and extreme lightheaded. . . . After that I had inform'd myself of the present circumstances and condition of his distemper—I retird with Dr. Gibbons and Dr. Hanns who were both there before to consult about his recovery before that I came, they had ordered him five blisters which were all put on, wee likewise ordered his Highness a drink to drink of, which was proper to suppress his loosness, which had its effect. . . . His feavor was a malignant feavour in all its symptoms, with a rash all over the body attending of it. Wee orderd him Cordial powders and Cordial Julops to resist the malignity, he tooke a paper of those powders that night which kept him in breathing sweats and brought out the rash in greater quantity, he had but very litle rest that night, accompanied with great sighings and dejection of spirits and towards morning complaind very much of his blisters. they were opend in the morning and they were drawn very well and run very well, and uppon the running of his blisters he was less light headed and the rash came out the more so that towards noon his head was considerably better, and his breath-

> ing freer, which gave us som encouragment at that time to hope his recovery. Wee orderd him in the evening two more blisters which were apply'd and to continue the method he was in, hopeing by the assistance of them and his other medcins he would have a better night, but before the blisters could take place the malgnity of the distemper retreating from the skin to the vital parts, he was of a suddaine after a little doseing taken with a convulsive sort of breathing, a defect in swallowing and a total deprivation of all sens which lasted about an hour and between twelve and one that night departed this life.
>
> JOHN RADCLIFFE

It is a melancholy tale, indeed, a moment of history which changed the entire course of empire.

If this was the best medical practice in all England, we can wonder how fared the common people. There were not a few observers who doubted the doctors' knowledge, skill, and good faith. No cynic expressed himself better than did Jonathan Swift, the ornament of Queen Anne's reign. Referring to doctors, Swift declared:

> Their fundamental is that all diseases arise from repletion, from whence they conclude that a great evacuation of the body is necessary, either through the natural passage or upwards at the mouth. Their next business is from herbs, minerals, gums, oils, shells, salts, juices, seaweed, excrements, barks of trees, serpents, toads, frogs, spiders, dead men's flesh and bones, birds, beasts and fishes, to form a composition for smell and taste the most abominable, nauseous and detestable they can possibly contrive, which the stomach immediately rejects with loathing; and this they call a vomit; or else from the same storehouse, with some other poisonous additions, they command us to take in at the orifice above or below (just as the physician then happens to be disposed) a medicine equally annoying and disgustful to the bowels; which relaxing the belly, drives down all before it, and this they call a purge or a clyster. For nature (as the physicians allege) having intended the superior anterior orifice only for the intromission of solids and liquids,

> and the inferior posterior for ejection, these artists ingeniously considering that in all diseases nature is forced out of her seat, therefore to replace her in it the body must be treated in a manner directly contrary, by interchanging the use of each orifice, forcing solids and liquids in at the anus, and making evacuations at the mouth.
>
> But besides real diseases we are subject to many that are only imaginary, for which the physicians have invented imaginary cures; these have their several names, and so have the drugs that are proper for them, and with these our female Yahoos are always infested.
>
> One great excellency in this tribe is their skill at prognostics, wherein they seldom fail; their predictions in real diseases, when they rise to any degree of malignity, generally portending death, which is always in their power, when recovery is not: and therefore, upon any unexpected signs of amendment, after they have pronounced their sentence, rather than be accused as false prophets, they know how to approve their sagacity to the world by a seasonable dose.[2]

Many practitioners were not at all clear in their own minds what they were doing or why they were doing it. But they were often quite eager to get more experienced advice. Hence arose the custom of consultation by mail, an accepted mode of practice in the seventeenth and eighteenth centuries. The usual procedure was for the practitioner to write some eminent physician, giving the salient facts and requesting an opinion. Many of these letters have come down to us. The celebrated Boerhaave had a large correspondence of this type. One excerpt will illustrate this practice.[3]

An apothecary wrote to Boerhaave:

> A young merchant twenty three years old, a very sober gentleman, not addicted to drinking or any other vice, fell suddenly into a spitting of blood in the month of March last year, without any external cause, and continued three days successively throwing up pure red blood by means of a gentle

cough. For sometime after the spitting returned four times a day, and then it was diminished in quantity for the two following days, being mixed with *Phlegm,* till at last all appearance of blood in it quite ceased, and there remained only a dry cough: he thought himself then in a fair way of recovery, as did all his friends, and his appetite was good, and he eated all kind of meat in his usual manner, not in the least complaining of indigestion. Yet some time after, he became lean and his flesh fell away, which his parents and friends could not so soon be sensible of, as he was always of a thin habit of body, and continued to mind his business with his usual alacrity. In the beginning of summer he tryed a milk diet for some months drinking goats milk and whey made from it, but without any benefit; in this state he continued till the month of *October,* at which time he was seized with a cough which was rather wet than dry, which he and his acquaintance imagined he had contracted from a *Cold* that was then epidemical, but contrary to all expectations it has remained with him till now, and throws up a tough viscid *Phlegm* of a green colour, the cough being likewise more frequent and violent: about the tenth of this present *January,* he fell into most profuse night sweating, attended with a great decay of his natural strength and loss of appetite.

During the whole course of the disease, he never complained of any pains in the breast or sides, his stools are every way as usual in health, and his urine is pale.

Thus, sir, I have given you a description of this worthy gentleman's case, as I had it from himself and friends. They wait with impatience for your advice, which will likewise oblige,

Sir, your most
devoted servant, etc.

To this Boerhaave wrote the following answer:

After carefully considering the case, I am of opinion that the patient labours under a real consumption, from a suppuration in the *Lungs* after the spitting of blood: the progress the disorder has already made, which is evident from the wasting of his strength, sweatings in the night, and decay of his flesh, denotes a very bad state. The fears of the present and future misfortunes are still the greater, that so good remedies as were hitherto tryed have proved ineffectual, and the disease gained ground notwithstanding the best advice. The best method I can propose, is, that the gentleman should ride as much as he can every day when his stomach is empty, and endeavor to increase his journeys by degrees; that he should go to bed at eight of the clock, and sleep in a bed well dryed in a room on one of the high floors; that he should rise betimes in the morning; his ordinary drink should be fresh *Mead,* mixed with equal quantity of milk; he may eat all kinds of grain prepared any how, soft herbs and greens, milk, river crabs, shelfish, and now and then be allowed a little fresh flesh. He ought to eat very sparingly at a time, and make the more meals.

Let him take every three hours in the day three of the pills A, drinking immediately after them three ounces of the liquor B. Let him likewise before he goes to bed, take off at once the draught C.

He may make a tryal of what I here propose for two months, to see if it will do any service. I pray God may bless it.

A.

Gumm. Ammon. drach ss.
Balsam. Peruv. gutt xv
Mastich.
Olibani
Opopanac. ana drach. ss.
Succi. Glycyrrh. inspissat, drach. iij.
Terebinthinae drach. ss.
M. F. Pilulae. gr. iij.

B.

℞ Recent. *Agrimon.*
Betonic.
Foenic.
Hyssopi.

Melissae
Veronic.
Virg. Aur. ana Man. ¼

These must be cut very small and infused in boiling water like The: let there be prepared xvi ounces every day.

C.

℞ *Balsam. Peruv.* gutt. iij
Ol. Amygdal. dulc. drach. iij
Syrup. Diacod. drach. v.
Vitelli ovi gr. xv.
M. F. Haustulus.

We can obtain some insight into the rationale of these prescriptions. The pills contained a mixture of resins, concerning which Boerhaave's own *Materia Medica*[4] offers the following information: Gum ammoniac was a resin which dissolved in water and which came from the East, although from what plant was not known. It was considered very good for asthmas and obstructions. Mastic, olibanum, and opopanox were also resins from the East. Mastic was detergent, astringent, and balsamic. It was excellent in consumptions and gleet. Olibanum was considered very good in disorders of the "breast" and lungs, as well as in the gleets. Opopanox had the virtue of ammoniac, as an attenuant and as an excellent remedy in asthma, but in large doses might purge a little. Licorice root was excellent for coughs and for all disorders of the lungs. Turpentine, of which four kinds were distinguished, was a balsamic, detergent, and diuretic and was considered good in gleets.

The herb ingredients of medicine B—agrimony, betony, fennel, hyssop, melissa, and veronica—were supposed to have rather indefinite virtues directed toward headache, nervous complaints, and obstructions. They served as cordials, stomachics, and cephalics.

Peruvian balsam and syrup of diacodion, not specifically described, had long been considered valuable, the first as an expectorant and stomachic when taken internally and the latter as a sedative.

We can, perhaps, imagine the emotions of the young man's family on learning of the great consultant's recommendations, and how eagerly the family would wait on the apothecary to get the prescriptions filled. Then we wonder what the apothecary did. Were all these drugs on hand? If so, how fresh were they? If not available, what did he do? How much similarity would the pills prepared by one apotheacry bear to those prepared by another? We may also ask, What difference did it make? and sympathize with Hahnemann's revolt against the materia medica of the time.

"Watering places" and mineral baths, very popular in the eighteenth century, played an important ancillary role in medical care. Contemporary feelings, however, were mixed. Some individuals displayed great enthusiasm, others were skeptical. Tobias Smollett, a physician first and then a novelist, condemned the whole practice of mineral baths. His one original medical contribution advocated the use of plain water rather than mineral waters. The essay included vivid descriptions. For example:

> Diseased persons of all ages, sexes, and conditions, are promiscuously admitted into an open bath, which affords little or no shelter from the inclemencies of the weather. . . . many of the fair sex are withheld by modesty from going into the Bath where they must not only mingle with male patients, to whose persons and complaints they are utter strangers; but, likewise, be exposed . . . to the eyes of all the company, in the Pump-room. . . . some may be apprehensive of being tainted with infectious distempers; or disgusted with the nauseating appearances of the filth, which, being washed from the bodies of the patients, is left sticking to the sides of the place.

> Although the king's and queen's Baths have been known to contain five-and-forty patients at one time, the number of guides does not exceed half a dozen of each sex; so that if any of those Bathers who are unattended should be ceased with a sudden *Vertigo,* fit, or other accident, they might lose their lives for want of proper assistance. . . . Some of them, on retiring from the Bath, must, from this defect in point of attendance, remain in the wet bathing dress, until their constitutions are greatly endangered. This inconvenience is rendered more grievious by the nature of that dress, which being made of canvass, grows cold and clammy in a moment, and clings to the surface of the body with a most hazardous adhesion.[5]

In his last novel, *Humphry Clinker,* Smollett, through his character Matthew Bramble, again attacked with his usual graphic style the whole institution of mineral baths and mineral waters. A few paragraphs are worth quoting:

> Two days ago I went into the King's bath by the advice of our friend Ch—— in order to clear the strainer of the skin, for the benefit of a free perspiration; and the first object that saluted my eye was a child, full of scrofulous ulcers, carried in the arms of one of the guides, under the very noses of the bathers. I was so shocked at the sight, that I retired immediately with indignation and disgust. Suppose the matter of these ulcers, floating on the water, comes in contact with my skin, when the pores are all open, I would ask you, what must be the consequence? Good heavens! The very thought makes my blood run cold. We know not what sores may be running into the water while we are bathing, and what sort of matter we may thus imbibe; the king's evil, the scurvy, the cancer, and the pox, and no doubt the heat, will render the *virus* the more volatile and penetrating. To purify myself from all such contamination, I went to the Duke of Kingston's private bath, and there I was almost suffocated for want of free air; the place was so small and the steam so stifling.
>
> After all, if the intention is no more than to wash the skin,

> I am convinced that simple element is more effectual than any water impregnated with salt and iron, which, being astringent, will certainly contract the pores, and leave a kind of crust upon the surface of the body. But I am now as much afraid of drinking as of bathing, for, after a long conversation with the doctor about the construction of the pump and the cystern, it is very far from being clear with me, that the patients in the Pump-room don't swallow the scourings of the bathers. I can't help suspecting that there is, or may be, some regurgitation from the bath into the cistern of the pump. In that case, what a delicate beverage is every day quaffed by the drinkers; medicated with the sweat, and dirt, and dandriff, and the abominable discharges of various kinds, from twenty different diseased bodies, par-boiling in the kettle below.[6]

And elsewhere in the same novel Matthew Bramble says:

> As for the water which is said to have effected so many surprising cures, I have drank it once, and the first draught has cured me of all desire to repeat the medicine. Some people say it smells of rotten eggs, and others compare it to the scourings of a foul gun. It is generally supposed to be strongly impregnated with sulphur; and Dr. Shaw, in his book upon mineral waters, says, he has seen flakes of sulphur floating in the well, *Pace tanti viri;* I, for my part, have never observed anything like sulphur, either in or about the well, neither do I find that any brimstone has ever been extracted from the water. As for the smell, if I may be allowed to judge from my own organs, it is exactly that of bilge-water; and the saline taste of it seems to be clear that it is nothing else than salt water putrefied in the bowels of the earth. I was obliged to hold my nose with one hand, while I advanced the glass to my mouth with the other; and after I had made shift to swallow it, my stomach could hardly retain what it had received. The only effect it produced was sickness, griping, and insurmountable disgust. I can hardly mention it without puking. The world is strangely misled by the affectation of singularity. I can not help suspect-

ing that this water owes its reputation in a great measure to its being so strikingly offensive.[7]

The voluminous medical literature of the eighteenth century included many case reports, usually leisurely and detailed, sometimes very dramatic and vivid. A superb example of this reporting we cull from Huxham; it presents an excellent case of hemorrhagic disorder, consistent with the modern concept of thrombocytopenic purpura, which Huxham deemed a "fever."

> An eminent Surgeon of a neighbouring Town, of a thin and somewhat tender Constitution, but constantly used to Action and Exercise, and frequently subject to Fevers, and scorbutic Rheumatisms, from taking Cold, etc. in *October* 1741, fell into a Kind of slow Fever, attended with slight Rigors, frequent Flushes of Heat, a quick weak Pulse, Loss of Strength and Appetite, with a great Load at his Breast, and a heavy Sort of Respiration.—Nothwithstanding this, he continued in his Business, constantly riding, and fatiguing himself for some four or five Days after this Seizure.—I met him at a Gentleman's House, who was my Patient; and finding him as above, and that his Breath was, even then, very offensive, I earnestly desired him to take timely and due Care of himself.—Two Days after, he, being at a Gentleman's in the Neighbourhood, was taken all on a sudden with a very great Faintness, and fell off his Chair: Upon lifting him up the Company observed several livid and violet-coloured Spots on his Arms and Neck. It was with very great Difficulty they got him Home, tho' but two or three Miles Distance, he very frequently fainting by the Way.—The Disorder encreased every Moment, he had a vast Langour with Pain and extream Oppression on the Praecordia, and a perpetual Sighing;—his Breath now stank abominably, and a foetid bloody Matter leaked continually from his Gums, and thousands of livid, violet and black Spots appeared all over his Body, on the Trunk, as well as the Limbs.
>
> He was bled to about ℥ xii from his Arm, but this gave him

no Manner of Relief, the Oppression, Sighing, Fainting, and Anxiety continuing as bad as ever, nay rather encreasing;—a violent Haemorrhage also broke forth from his Nose; which continuing from both Nostrils, he was bled again to ℥ x about twelve Hours after the former Bleeding:—neither did this give him any Relief, but encreased his Weakness considerably, and he continued as anxious, restless, and oppressed as ever, without even the least Sleep. The Blood now not only issued from his Gums and Nose, but he also coughed up Blood.—Indeed the Bleeding from his Nose had ceased somewhat, but it encreased from his Gums, and in a surprising Manner Blood now likewise dropped, tho' slowly, from the Caruncle of one of his Eyes; and several livid Pustules on his Tongue, and withinside his Lips, broke, and discharged a bloody, thin Matter very copiously.

The Haemorrhage being somewhat restrained, a bloody Dysentery came on with severe Gripes, an excessive Faintness, and he was still exceeding restless and very feverish: his Pulse now intermitted every sixth or eighth Pulsation, and then fluttered on again vastly quick; he had likewise a constant *Tremor* and *Subsultus*.—The Haemorrhage all this while continued from one Part or other, and when stopped at one Place forthwith burst out at another; so that his Urine now seemed tinged with Blood, being very dark-coloured, nay almost black. Soon after he was bled the second Time, I was sent for, and hastened to him.—I found him in the Manner described, under an inexpressible Anxiety, yet quite free from a Delirium, though he had no Manner of Sleep for several Days and Nights: His Tongue was vastly black, and his Breath so insufferably stinking, that it was greatly offensive even at a considerable Distance; and his Stools were so horribly nauseous and foetid, that the very Nurses fell into Vomitings and Faintness in carrying them off.

I found that neither of the Portions of the Blood that had been drawn (not even the first) had separated into *Crassamentum* and *Serum* as usual, tho' the former had stood so many Hours; but continued as it were half coagulated, and of a bluish livid Colour on the top:—it was most easily divided by the slightest Touch, and seemed a *purulent Sanies* rather than Blood, with a Kind of a *Sooty Powder* at Bottom. His Haemorrhage still continued, especially from the Tongue, Lips and Gums, with a perpetual Dripping of thin bloody Ichor from his Nose; so that he was reduced to an extream Degree of Weakness, with neverceasing Tremblings, *Subsultus Tendinum,* and almost continual Faintings.

What was to be done in this dreadful Case? Would the hot, alexipharmac, volatile Cordials and Blisters have served him, as some might have imagined, considering his extream Weakness, Faintings, Load on the Praecordia, Tremblings, etc? But would they not have been certainly *deleterious,* would they not have certainly killed him? as they would have added to the stimulating Acrimony, encreased the Fever, and further destroyed the *Crasis* of the Blood, already nearly quite dissolved, and reduced to a Kind of *putrid Gore.*

I took it in this View, and, as I had experimentally and repeatedly known the great Use of the *Bark* in preventing and stopping the Advance of Gangrenes, I gave him frequently of it in small Doses with *Elixir Vitrioli,* premising a small Quantity of *Rhubarb.*—Besides this he drank Tincture of Roses, with Cinnamon Water, made very acid, and also a Decoction of *Sevil* Orange Rind, red roses, Cinnamon, and a little *Japon* Earth (as it is called) well acidulated: Claret, and red Port, with about half Water, he drank at Pleasure.—As the *Bark* sat easy with him, I continued its Use, and encreased its Quantity, giving with it some *Confect. Fracast. sine melle* to restrain the dysenteric Flux;—and yet I now and then interposed a small Dose of *Rhubarb,* to carry off any bloody, bilious, or sanious Matter that might be lodged in, or leak into the Intestins. In the mean Time I ordered him to be frequently supported with Rice, Panado, Sago, Jellies of Hartshorn well acidulated, Toast out of Claret, or red Port Wine; and I directed Fomentations of Aromatics and Astringents, boiled in red Wine, to be frequently applied to the whole Abdomen.

By this Method, steadily persisted in, was this poor Gentleman, thro' divine Goodness, raised from a State of universal

Rottenness, as it were, to perfect Health: Not but that, for a very considerable Time after his Fever was quite gone off, he continued extremely weak; and even after he was capable of walking abroad, the Haemorrhage from his Nose would return on the least Occasion, his Gums would bleed on the slightest Rubbing, and his Breath continued very offensive for a long Time.—By the further Use of the *Cortex, Elix. Vitrioli,* etc. this also intirely ceased:—But his Legs and Feet continued very much swoln for a much longer Time, and his Flesh all over the whole Body remained exceedingly soft, tender, and sore, scarce bearing the least Touch.—Rhabarbarate Purges, easy stomachic Chalybeates, Elixir of Vitriol, *Pyrmont* Water with proper Diuretics, and gentle regular Exercise at length carried off all those Symptoms; and in about two or three Months he recovered a good State of Health, which he still enjoys.[8]

Published medical reports have a certain formality, a dressed-up air of best-foot-forward. Rather different are the medical documents not intended for publication, which merely chronicle the average practice of average doctors. A few years ago there came into my possession a manuscript recording the day-by-day activity in a British dispensary.[9] This journal, a ledger volume of some 260 pages, covers the period from June 1, 1787, to January 25, 1788. Internal evidence points to a Scottish locale—the patients' names were chiefly Scottish—Campbell, Burns, Stewart, Cameron, Barrie, and the like—although some were of Irish extraction; and occasionally it is noted that patients have been to Glasgow or Edinburgh, while no other large city is mentioned.

The manuscript entries differ in handwriting and in style—some entries are very informative, others exceedingly laconic. Under each date appear the patients' names and various medical data. Brief notes on new patients give age, pulse rate, a few words of history, a word or two concerning the physical findings, and then the medicines prescribed. Follow-up visits may or may not include genuine progress-reports but always show the further medicines given. Many cases are followed to the final outcome —either death or recovery—but all too often, after few or many entries, the patient simply drops from sight.

Infectious disease formed a major part of the practice. Many times whole families were affected. There is, for example, the chronicle of the Mann family, to the understanding of which the dates are particularly important. Little Alex, aged eight, was first seen on July 6. He had a "typhus," of eight days' duration. His pulse was 112, weak and irregular; his bowels were regualr; he was noted to have great thirst and debility, with some delirium. Treatment included ammonium acetate (spirits of mindererus) and tincture of opium (thebaica), and a blister was applied between the shoulder blades. By July 8 the blister had "risen and discharged well," and his symptoms were better. On July 10 his pulse was 120, skin dry, thirst increased, and there was some delirium, while by July 13 he had a severe cough. Nevertheless, with Peruvian bark, red wine, and a "pectoral mixture," he improved, and by July 17 his symptoms of fever were gone, although he had some "looseness." On July 12 Elizabeth Mann, aged twelve, was noted to have had a fever for seven days past. Her tongue was dry, skin parched and hot, pulse 100, and the next day she had severe diarrhea and some delirium. Although her course was stormy, with restlessness and delirium, she showed slow improvement until, by July 30, she was "greatly better." Meanwhile, Alex Mann, Sr., aged forty-six had made his appearance July 13 with "a Typhus these few days past for which he got a vomit from Y^e Dispensary. Pulse 96 Debility and Stupor." He became rapidly worse. July 15, "Pulse 108 Stupor and delirium Continues bad appearance of his eyes—skin was last night covered with Petechiae, but mostly gone today." By the seventeenth his pulse was very weak and fluttering, debility increased, his skin was again covered with petechiae, and that night he died. There was no autopsy.

Then, on July 22 the "Widow Mann," aged forty, began to complain of weakness, anxiety, "and other symptoms of beginning Typhus." Her pulse was 100. There was some diarrhea and she had complained of a "remarkable disagreeable smell when assisting in lifting the dead body of her husband which likewise affected another woman in the same manner, who now has also got the fever." But Widow Mann, whatever her disease, showed a very different clinical course. With Peruvian bark and opium she improved very rapidly. By July 23, that is, only two days after onset, her symptoms were "almost gone," and on the twenty-fourth she was dismissed as cured.

There were numerous families showing similar progression of various febrile illnesses, although death was very infrequent. Most cases, however, were not of epidemic character. Among the infectious diseases tuberculosis in one or another form is repeatedly noted. There was frank "consumption." For example, a twenty-one-year-old widow complained of "general soreness over her body, pain in her bones difficulty of breathing with great debility cold sweats in time of sleep which greatly weaken her, severe cough chiefly in the night time. . . . Eyes have a consumptive appearance." Or another girl, only sixteen, who "about seven months ago was seized with a sever [sic] cough—pain of the breast—dyspnea—night sweats —occasional diarrhea—copious expectorations and all the symptoms of phthisis."

There are also many references to "scrophulous" swellings. One patient was a thirty-year-old man who "for two years past has laboured under a scrophulous affection of the wrist and back of the left hand which is now degenerated into a number of fungus ulcers and spongyness of the carpal and metacarpal bones. He has likewise been afflicted w^t a severe pain of the small of his back probably owing to an affection of the mesenteric glands. This last complaint is of 3 or 4 years standing. Pulse small and feeble—Face pale—Body Emaciated." Or the nineteen-year-old girl who "for 13 years past has been troubled w^t Scrophulous running upon her left thigh and leg which is greatly drawn up—at present there is a large tumor upon her thigh which appears to be in a state of suppuration pulse 100."

Gonorrhea was well recognized. Thus, Ann Wallace, "Aged 32. About 6 months ago was affected w^t Gonorrhea attended w^t pain and heat in making her water for which she got several medicines since when the heat and pain in making water have gone but the running continue." For injection this mixture was prescribed: calomel, sugar of lead, white vitriol, "crabs eyes," (oculi cancrorum precipitati), opium and gum arabic. We do not learn, however, whether this was successful. Another patient, a fourteen-year-old girl about whom it is said only, "For two months past has been affected w^t a virulent gonorrhea," received a much more prosaic remedy, namely, oil of sweet almonds and mucilage gum arabic for injection, and the next day, opium and extract of lead in gum arabic. Two weeks later she is noted to be "better" and then is followed no more.

Syphilis is not mentioned by name but is referred to as a disease "of venereal nature." A thirty-three-year-old man "has complained for 3 years past of a sore throat which was better times but is again much worse and the uvula is a good deal decayed. Complains much of costiveness and restlessness in the night time. It appears to be of a venereal kind." He received mercurial pills, and two weeks later, "Throat looks better but thinks the pills begins to affect his mouth." A few days later, "Ulcer better but complains much of faintishness." Further information is not given.

Other forms of disease are abundantly represented. Some cases indicate renal impairment. For example, a six-year-old girl for the previous four months "has been subject to dropsy her belly at present is swelled to a remarkable degree—great thirst, belly bound makes little or no urine and for 3 weeks past has been obliged to rest upon her knees and elbows." For this condition scilla was prescribed. Another girl of fourteen "for 4 months past has been affected w^{t} ascites, complains of great pain in her bowels and difficulty of breathing makes no water. Pulse quick and feeble." Here, in addition to scilla, the doctor prescribed jalap and cream of tartar, and also eight ounces of aqua vitae, "a little to be mixed w^{t} Juniper tea and given for common drink."

There are some vivid examples of arthritis. A thirteen-year-old boy "for several days past has been afflicted w^{t} violent Rheumatic pains in his legs and arms which have almost deprived him of the use of them they were brought on by standing in the river some hours and neglecting to shift afterwards. Skin hot P. 100 and full belly bound." Medication here consisted of elixir of guaiac, wine of antimony, and tincture of opium. Further prescriptions included calomel, scilla and jalap, and powdered rhubarb. Eleven days after the first entry, we find the progress note "Better." Improvement continued slowly, and after three weeks we find "℞ Oppodeldoch oz. ii Sig. To rub his knee w^{t}." Apparently the polyarthritis resolved, leaving only one knee still painful.

Two cases note gastro-intestinal bleeding, presumably peptic ulcer, or perhaps varices. The first was a forty-four-year-old woman first seen on October 30 with rather non-committal symptoms. "Since Friday last has been affected w^{t} great pain in her breast and breathlessness likewise pain of her head great thirst belly bound." On November 13 there was a marked change: "Complains of sickness after her meals particularly her breakfast the more so if her food is liquid. On Friday last w^t a dose of salts she passed a large quantity of a liquid as black as ink which she did last year twice the first time black and second of the colour and consistence of blood." Nevertheless, the doctor continued the cathartic salts. It is significant that this is the last entry for this patient, though the book continues for two-and-one-half months. We might guess, perhaps, that she consulted another physician.

The second case was a forty-five-year-old woman who "about a twelve month ago was attacked w^t a vomiting of blood. Since which time she has had three returns of it. Three days ago it attacked her again the stools are of a black colour—has no cough nor pain of her breast but complains of a pain in her left side which has continued more or less this year and half. Pulse 96 and rather feeble —Tongue a little furred belly natural." Four days later, "The vomiting of blood has disappeared. She complains of the menstrual flux being small quantity—is much troubled w^t erratic pains and flatulence. Pulse 80 and feeble. Tongue foul belly much constipated." To correct this she received castor oil and tincture of senna. Apparently no harm ensued. The book comes to an end after recording one further visit, which is not contributory.

These few excerpts from a record not designed for publication offer a true glimpse of eighteenth-century medical practice. There were concrete everyday problems which had to be met. Medical theory was not important. Physiologic and pathologic observations were not very relevant. Instead, there was a frankly empirical practice, and most patients got well, even as they do today.

The practitioners of the latter eighteenth century had numerous texts to guide them. There were the scholarly volumes, which discussed theory as well as practice, and there were the various manuals, which were short, dogmatic, and empirical, and which gave more or less detailed and specific instructions. The title page of one such volume is quite self-explanatory: "*The London Practice of Physic:* Wherein the Definition and Symptoms of Diseases, with the Present Method of Cure, are clearly laid down. With proper Tables Exhibiting the Doses of Medicines and the Quantity of Purgatives, Opiates, and Mercurials, in the Compositions of the London Dispensatory; and Complete Index of the Diseases and Medicines. The whole calculated for the General Use of Young Practitioners. The Fourth Edition. With large Additions and Amendments. Dublin, Printed by James Williams, 1779." This particular edition was apparently pirated.

The two following excerpts quote the entire text of the respective disorders:

OF THE ANGINA, OR QUINSEY

An angina is an inflammation of the throat, with burning heat, pain, tumour, and redness; a constant inclination to, and difficulty in deglutition, attended with a fever. Frequently the uvula and parts adjacent are highly relaxed and inflamed, and liquids often rejected by the nose, with redness in the face.

Here the diet should chiefly consist of water-gruel, weak whey, barley-water, and chicken-broth, drank warm.

Bleed largely, if the violence of the symptoms or fever requires it, and repeat it according to the exigency of the case: then

Potio Purgans.

℞ Infus. senae ʒ iii
Tinctur. senae ʒ vi.
Tartar. solub. ʒ ss. f. potio statim sumenda.

If the symptoms are severe, apply blisters as near as possible to the parts affected, and to the back, and use this gargle:

Gargarisma Ammoniacale.

℞ Decoct. pectoral ʒ vi.
Spirit. sal. ammoniac. ℥ ss. m.

A flannel well moistened with liniment. volat. is recommended by some; or the bread and milk poultice, with ʒ ij. or ℥ iij. of camphor.

Through the whole course of the disease purge gently, or throw up lenient clysters till the inflammation abates; then prescribe the following gargarism:

Gargarisma Vitriolicum.

℞ Tinct. rosar. ʒ vi.
Mel. rosar. ℥ ss.
Spir. vitriol, ten. gutt. xx. m.

Antimonials have frequently been of great service in this disorder.

If a suppuration should come on, forbear evacuations, and order the vapour of emollient plants to be received into the throat through a funnel; and when the maturation is complete, puncture the abscess so that the matter may be discharged.

After which you may add tincture of myrrh to the last mentioned gargle, and finish the cure by prescribing the bark, a milk diet, and gentle exercise.

In cases of great danger, where the respiration is much affected, the steam of hot vinegar, drawn in with the breath, does vast service; and where a suppuration has taken place, a vomit of ten grains of white vitriol sometimes breaks the tumour and snatches the person from the jaws of death.[10]

OF THE PUERPERAL FEVER

The puerperal fever is a disease peculiar to lying-in women; and though it happend so frequently, yet it hath, till of late, been strangely overlooked by authors, even those who have written professedly on fevers.

It commonly begins with a rigor or chilliness, on the first, second, or third day after delivery; followed by a violent pain, and soreness, over the whole hypogastric region. There is much thirst; pain in the head, chiefly in the forehead, and parts about the eyebrows; a flushing in the face; anxiety, a hot dry skin; quick and weak pulse, though sometimes it will resist the finger pretty strongly; a shortness in breathing; high-coloured urine; and suppression of the lochia. Sometimes a vomiting and purging attend from the first, but in general, in the beginning, the belly is costive: however, when the disease proves fatal, a diarrhœa generally supervenes, and the stools at last become involuntary.

There have been several treatises written lately upon this disorder; but as the authors are not agreed as to the cause of the complaint, nor the proper mode of treating it, I shall not pretend to give any precise method of cure. In general, the good sense of the physician will teach him that, when the pulse is strong, and the pain, heat, and thirst, are great, bleeding, laxatives, and the antiphlogistic regimen will be requisite; on the contrary, when there is great debility, the patient must be supported, and all evacuations carefully avoided.[11]

When a volume of the above character was at hand, it was not too difficult to practice medicine. It was requisite, however, to be well versed in bloodletting. A tremendous literature had grown around this subject, and there were certain differences of opinion. A fairly representative point of view we find in William Buchan's *Domestic Medicine:*

OF BLEEDING

No operation of surgery is so frequently necessary as bleeding; it ought, therefore, to be very generally understood. But though practised by midwives, gardeners, blacksmiths, etc. very few know when it is proper. Physicians themselves have been so much the dupes of theory in this article, as to render it the subject of ridicule. It is an operation of great importance, and must, when seasonably and properly performed, be of singular service to those in distress.

Bleeding is proper at the beginning of all inflammatory fevers, as pleurisies, peripneumonies, etc. It is likewise proper in all topical inflammations, as those of the intestines, womb, bladder, stomach, kidnies, throat, eyes, etc. as also in the asthma, sciatic pains, coughs, head-achs, rheumatisms, the apoplexy, epilepsy, and bloody flux. After falls, blows, bruises, or any violent hurt received either externally or internally, bleeding is necessary. But in all disorders proceeding from a relaxion of the solids, and an impoverished state of the blood, as dropsies, cacochymies, etc. bleeding is improper.

Bleeding for topical inflammations ought always to be performed as near the part affected as possible. When this can be done with a lancet, it is to be preferred to any other method; but where a vein cannot be found, recourse must be had to leeches or cupping.

The quantity of blood to be let must always be regulated by the strength, age, constitution, manner of life, and other circumstances relating to the patient. It would be ridiculous to suppose that a child could bear to loose as much blood as a grown person, or that a delicate lady should be bled to the same extent as a robust man.

From whatever part of the body blood is to be let, a bandage must be applied between that part and the heart. As it is often necessary, in order to raise the vein, to make the bandage pretty tight, it will be proper in such cases, as soon as the blood begins to flow, to slacken it a little. The bandage ought to be applied at least an inch, or an inch and half from the place where the wound is intended to be made.

Persons not skilled in anatomy ought never to bleed in a vein that lies over an artery or a tendon, if they can avoid it. The former may easily be known from its pulsation or beating, and the latter from its feeling hard or tight, like a whip-cord under the finger.

It was formerly a rule, even among *regular* practitioners, to bleed their patients in certain diseases till they fainted. A more ridiculous rule could not be proposed. One person will faint at the very sight of a lancet, while another will loose almost the whole blood of his body before he faints. Swooning depends more upon the state of the mind than of the body; besides, it may often be occasioned or prevented by the manner in which the operation is performed.

Children are generally bled with leeches. This, though some-

> times necessary, is a very troublesome and uncertain practice. It is impossible to know what quantity of blood is taken away by leeches; besides, the bleeding is often very difficult to stop, and the wounds are not easily healed. Would those who practice bleeding take a little more pains, and accustom themselves to bleed children, they would not find it such a difficult operation as they imagine.
>
> Certain hurtful prejudices, with regard to bleeding, still prevail among the country people. They talk, for instance, of head-veins, heart-veins, breast-veins, etc. and believe that bleeding in these will certainly cure all diseases of the parts from whence they are supposed to come, without considering that all the blood-vessels arise from the heart, and return to it again; for which reason, unless in topical inflammations, it signifies very little from what part of the body blood is taken. But this, though a foolish prejudice, is not near so hurtful as the vulgar notion, that the first bleeding will perform wonders. This belief makes them often postpone the operation when necessary, in order to reserve it for some more important occasion, and, when they think themselves in extreme danger, they fly to it for relief, whether it be proper or not. Bleeding at certain stated periods or seasons has, likewise, bad effects.
>
> It is, likewise, a common notion, that bleeding in the feet draws the humours downwards, and consequently cures diseases of the head and other superior parts: but in all topical affections, blood ought to be drawn as near the part as possible. When it is necessary to bleed in the foot or hand, as the veins are small, and the bleeding is apt to stop too soon, the part ought to be immersed in warm water, and kept there till a sufficient quantity of blood be let.[12]

Perhaps the best epitome of eighteenth-century medicine we find in the rise and fall of inoculation. This method of protecting against the smallpox was introduced into England early in the eighteenth century. After many difficulties the practice became relatively popular until it underwent total and permanent eclipse from the newer vaccination which Jenner described in 1798.

The main features of variolation are well known. There was a deliberate inoculation with smallpox, in the hope that the recipient would have a light attack that would nevertheless convey lasting protection. This custom had been prevalent in the East for centuries. Lady Mary Wortley Montague had had her own son successfully inoculated in Constantinople in 1717. Returning to England in 1722, she was instrumental in introducing the procedure into Great Britain, over considerable opposition.

There were unsung heroes who mediated the success—felons condemned to death, for whom the Princess of Wales secured a pardon on the condition that they submit to inoculation. The difficulty was not to persuade the felons, for they had nothing to lose, but to persuade a physician to perform the operation. Five of the condemned men contracted the smallpox "favorably." A sixth, who had previously had natural smallpox, remained well. All escaped hanging, and variolation, for better or worse, was launched in Great Britain.

There were various technical methods. One which achieved considerable vogue was that of a Dr. Nettleton. He made incisions about one inch in length in one arm and in the opposite leg. Into these wounds he introduced cotton, charged with variolous pus and held in place with plaster. Although foul ulcers developed, these were well regarded, since the discharge supposedly helped evacuate the peccant matter.[13]

The operation carried considerable danger. Of those inoculated in the early days, one in fifty to one in sixty died of the smallpox. Moreover, it came as quite a surprise that smallpox, deliberately induced, was just as infectious as the natural disease. Although their own infection might be light, inoculated patients readily disseminated the disease. The incidence of smallpox in the general population rose sharply. The procedure was widely condemned. It lost favor during the 1730's but regained popularity when substantial successes were reported from the British colonies. In 1754 the College of Physicians strongly approved inoculation, and thereafter opposition declined.

Methods of inoculation became very complicated. Originally the transfer of smallpox from donor to patient was a relatively simple affair, but gradually the preparatory treatments and medications increased, until inoculation became a very complex business indeed. Preparatory treatments alone might require a month, and then the inoculation sites were transformed into draining sores that might remain for five to six weeks. The whole procedure, with the necessary purgatives, emetics, bleeding, blisters, opiates, and nervous drugs, was very profitable to the physicians.[14] Only the rich could afford to be inoculated.

The grave social problems raised thereby brought certain reactions. Considerable agitation developed to provide the benefits of inoculation to the poor. And at the same time much less drastic techniques were developed. It was Daniel Sutton, using a vastly simplified technique, who established virtual mass inoculation with much lower mortality. And incidentally he acquired a substantial fortune.

Sutton employed secret remedies, which supposedly accounted for his success but which were soon shown to be essentially antimony and mercury. Sutton kept his method more or less secret and did not publish his technique and results until 1796, in a volume which is still excellent reading today.[15] As early as 1767, however, Thomas Ruston published the simplified techniques and the Suttonian medicines.[16] During the 1760's Thomas Dimsdale also studied Sutton's methods and became even more famous as an inoculator. Dimsdale published his own book in 1767,[17] and the next year he was summoned to Russia to inoculate the Empress Catherine and her famiy. The prestige so gained rendered him easily the foremost inoculator in Great Britain, although Sutton was still very high in popular esteem.

There was a host of imitators, for inoculating was a very profitable occupation. There was also considerable controversy among medical men. The tensions of the 1760's are well reflected in Buchan's *Domestic Medicine,* the first edition of which was published in 1768. Buchan felt that inoculation was a great boon to mankind, and should be universally practiced. He wrote as follows:

> No discovery can ever be of general utility while the practice of it is kept in the hands of a few. . . . The fears, the jealousies, the prejudices, and the opposite interests of the Faculty, are, and ever will be the most effectual obstacles to the progress of any salutary discovery. Hence it is that the practice of inoculation never became, in any measure, general, even in England, till taken up by men not bred to physic. These . . . by acting under less restraint than the regular practicioners, have taught them that the patient's greatest danger arose, not from the want of care, but from the excess of it.
>
> They know very little of the matter, who impute the success of modern inoculators to any superior skill, either in preparing the patient or communicating the disease. Some operators, indeed, from a sordid desire of ingrossing the whole practice to themselves, pretend to have extraordinary secrets or nostrums in preparing persons for inoculation, which never fail of success. But this is only a pretence calculated to blind the ignorant and inattentive. Common sense and prudence alone are sufficient both in the choice of the subject and management of the operation. . . .[18]

An interesting commentary on eighteenth-century medical economics and medical ethics, both so intimately connected with medical practice.

Buchan, convinced that inoculation should be universal, recommended that parents should themselves perform the operation on their children. He hoped that soon "parents will think no more of inoculating their own children than at present they do of giving them a dose of physic."[19] And furthermore the clergy should inoculate their parishioners. Most of the clergy, he declared, "know something of medicine. Almost all of them bleed, and can order a purge, which are all the qualifications necessary for the practice of inoculation."[20] The actual introduction of variolous matter was made as simple as possible—a very minute superficial incision, or even an impregnated thread laid on the arm and covered with plaster. When the disease supervened, treatment took the form of a cooling regime, a light diet, diluting drinks, and gentle purging.

Buchan's views, first expressed in 1768, remained virtually unchanged in the numerous editions, British and American, that appeared during the remainder of the century. The American edition of 1799[21] showed very little change, while vaccination was not even mentioned. But in later editions the impact of Jenner's great discovery had made itself felt, and by 1808 the entire picture had changed. In that edition there was only a very short section on inoculation but an extensive discussion of vaccination. The editor admitted the failure of inoculation. Since the practice, far from being universal, was confined to only a few, it was, he declared, actually harmful; it spread the disease to many who otherwise might never have had it. The incidence of smallpox was greater at the end of the century than it was before inoculation was introduced. All possible benefit was lost because it had not been extended to the whole community. "Hence it would appear that inoculation has done a great injury to society at large."[22] Buchan pointed out, however, that in the few years Jennerian vaccination had been used, the mortality from smallpox had dropped sharply—in London, from 2,409 deaths in 1800 to 622 in 1804; in Vienna from 835 in 1800 to only 2 in 1804.

Eighteenth-century medical practice witnessed many changes but none, perhaps, so complete as the final and relatively sudden fall of inoculation. Along other fronts changes were more gradual. In this chapter we have seen a few representative segments of medical practice, composed, it might seem, of absurd errors and faulty theory, cemented by human failings. So it was, but so too will our boasted twentieth-century triumphs appear to our descendants two hundred years hence. The practice of medicine changes constantly, just as does the art of painting or of architecture. But the soul of the artist does not significantly change, nor does the soul of the doctor.

Notes

CHAPTER I

1. Charles Goodall, *The Royal College of Physicians of London, Founded and Established by Law; as appears By Letters Patents, Acts of Parliament, adjudged Cases, etc. and an Historical Account, of the College's proceedings against Empirck and unlicensed Practicers in every Prince's Reign from their first Incorporation to the Murther of the Royal Martyr, King Charles the First* (London, 1684), pp. 1–2.
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3. *Ibid.*, p. 12.
4. *Ibid.*, p. 5.
5. *Ibid.*, pp. 18, 19.
6. *Ibid.*, pp. 119 ff.
7. J. Cordy Jeaffreson, *A Book about Doctors* (New York, 1851), pp. 69, 70.
8. *Medicina Flagellata, or, The Doctor Scarify'd. Laying open the Vices of the Faculty, the Insignificancy of a great Part of their Materia Medica, with certain Rules to discover the true Physician from the Empirick, and the Useful Medicine from the Noxious and Trading Physick* (London, 1721), p. 59.
9. *Ibid.*, p. 63.

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11. Alexander Pope, *An Essay on Criticism,* Part I, ll. 108–11.
12. C. R. B. Barrett, *The History of the Society of Apothecaries of London* (London, 1905), p. 115.
13. *Medicina Flagellata,* p. 100.
14. Pitt, *op. cit.,* p. 12.
15. Graham Everitt, *Doctors* and *Doctors: Some Curious Chapters in Medical History and Quackery* (London, 1888), pp. 51, 52.
16. Pitt, *op. cit.,* Preface.
17. *Medicina Flagellata,* p. 10.
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19. Pitt, *op. cit.,* p. 18.
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22. *Ibid.,* p. 65.
23. *Ibid.,* p. 95.
24. *Ibid.,* p. 89.
25. *Ibid.,* p. 108.
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27. Phyllis Allen, "Medical Education in 17th Century England," *Journal of the History of Medicine,* 1 (1946), 115–43.
28. William C. Wells, "A Letter to the Right Hon. Lloyd, Lord Kenyon, relative to some conduct of the college of physicians of London, posterior to the decision of the court of king's bench in the case of Dr. Stenger; and containing observations on a principal ground of that decision (July 1, 1799)," *Two Essays: One upon Single Vision with Two Eyes; the Other on Dew* (London, 1818), pp. 395–96.
29. Ernest Gray, *The Diary of a Surgeon in the Year 1751–1752* (New York, 1937).
30. John D. Comrie, *History of Scottish Medicine* (2d ed.; London, 1932), I, 271, 272.
31. "An Enquiry into the Present State of Polite Learning," *The Works of Oliver Goldsmith* (4 vols.; London, 1854), II, 64-65.

CHAPTER II

1. John Wesley, *Primitive Physick; or, an easy and natural method of curing most diseases* (2d ed.; Bristol, [1747]).

2. *Ibid.*, p. 115.
3. *Ibid.*, p. 90.
4. *Ibid.*, p. 39.
5. George Dock, "The 'Primitive Physic' of Rev. John Wesley: A Picture of Eighteenth Century Medicine," *Journal of the American Medical Association,* 64 (1915), 629–38.
6. W. R. Riddell, "Wesley's System of Medicine," *New York Medical Journal,* 99 (1914), 64–68.
7. George Berkeley, "Siris: A Chain of Philosophical Reflexions concerning the Virtues of Tar-Water," *The Works of George Berkeley, D.D.,* ed. Alexander Campbell Fraser (4 vols.; Oxford, 1901), III, 168.
8. Berkeley, "Letter to Thomas Prior, Esq., Containing Some Farther Remarks on the Virtues of Tar-Water, and the Methods for Preparing and Using It," *Works, III,* 306.
9. Berkeley, "Siris," p. 167.
10. Berkeley, "Letter to Thomas Prior," p. 311.
11. Berkeley, "Siris," p. 170.
12. Berkeley, "A Second Letter to Thomas Prior, Esq., On the Virtues of Tar-water," *Works, III,* 320.
13. Berkeley, "Farther Thoughts on Tar-Water," *ibid.,* p. 343.
14. Berkeley, "Siris," p. 161.
15. Ben Jonson, *Volpone,* Act II, scene 1.
16. *The Life and Extraordinary History of the Chevalier John Taylor . . . Written from authentic materials, and published by his son, John Taylor, Oculist* (2 vols.; London, 1761), I, 81–108.
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18. *The Spectator,* No. 444 (July 30, 1712).
19. "Some Famous Quacks. V. Sir William Read," *Practitioner,* 78 (1907), 416–21.
20. Graham Everitt, *Doctors* and *Doctors: Some Curious Chapters in Medical History and Quackery* (London, 1888), pp. 254, 255.
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22. *Ibid.*, p. 516.
23. See n. 16, above.
24. "Some Famous Quacks. IV. Joshua Ward," *Practitioner,* 78 (1907), 278.
25. J. Cordy Jeaffreson, *A Book about Doctors* (New York, 1851).
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